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THE BRITISH JOURNAL OF PSYCHOLOGY

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Volume I. 1904—1905

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UME I JANUARY, 1904

THE BRITISH
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EDITORIAL.

PSYCHOLOGY which till recently was known among us chiefly as Mental
hilosophy and was mainly concerned with problems of a more or less specula-
ve and transcendental character, has now at length attained the position of
positive science; one of special interest to the philosopher, no doubt, but
ill independent of his control, possessing its own methods, its own specific
roblems and a distinct standpoint altogether its own. 'Ideas' in the
hilosophical sense do not fall within its scope: its inquiries are restricted
ntirely to 'facts.' In pursuit of these it is brought into close relations
ith biology, physiology, pathology and again with philology, anthropology
and even literature. Its results also have important practical applications
for the educationist, the jurist, the economist, quite apart from their
oretical bearing on the problems of the epistemologist and the moralist.
ming a distinct science it has thus increased the intimacy and variety
ns with other sciences manifold.

his increased importance which Psychology has now
mentioned:—First, within the last fifteen
d in other countries—all still

Psychologists held its first meeting in Paris in 1889, its second in London in 1892, and has met quadrennially in various places since. The growth of psychological literature too may be seen in the fact that the number of entries in the first Annual Bibliography published (that of the *Zeitsch* *für Psychologie und Physiologie der Sinnesorgane* for the year was 1325, while the number for 1901, the latest published, amou 3624. Among these entries are to be found several dozen text-boo Psychology in English alone.

No wonder then that—as was stated in our preliminary circular—" belief is widespread that the time has come for starting an English journ devoted exclusively to Psychology in all its branches," analytical, geneti comparative, experimental, pathological, individual, ethnical, &c. Accordingly, to meet this want the publication of the present Journal has been undertaken. Within the vast range of topics just indicated it will side with no school and have no predilections. Its one aim will be to serve as the 'organ' of all alike who are working at any one of the many branches into which Psychology has now differentiated. Those which flourish most will naturally demand—and receive—the most space. Among the standards by which communications will be judged that of length will not be one. Short notes, observational, critical or historical, will be welcomed along with elaborate papers, setting forth by the aid of tables and diagrams the details of lengthy experimental investigations. But in every case, in the interests of their readers not less than in their own, the Editors are resolved stringently to insist on conciseness and relevancy of statement. Besides original articles the Journal will contain from time to time critical discussions of curre controversial topics whether arising out of articles in journals or in co quence of the appearance of new books of importance. Bu undertake regularly to review books, or to furnish abstra of other journals, or to publish bibliographical important matters, but they seem isting periodicals.

ON THE DEFINITION OF PSYCHOLOGY.

BY JAMES WARD.

Three views of Psychology—that of Aristotle, starting from the living organism; that of Descartes, starting from Mind as a res cogitans; *and that synthesising these in the conception of 'immediate experience.' Influence of the Cartesian dualism and the restriction of psychology to the highest level of self-consciousness on its subsequent developments. Ambiguity of 'subjective' and 'objective' as applied to experience. Immediate experience is both and is primarily conative not cognitive. Objections to defining psychology by reference to consciousness: Presentationism. Analytic psychology must precede genetic.*

EVERYBODY can tell [illegible] general way what psychology is about: in fact there is perhaps no science the subject-matter of which can be more clearly and promptly set forth in popular language and for practical purposes. For the student of history or of biography, for the physician or the educationist, it is enough to know that psychology will furnish him with a description of normal mental processes and their development, more or less incomplete, no doubt, but systematic as far as it goes. The moment, however, that we attempt to render approximate definitions of this sort precise—whether by clearing the conception of 'mental processes' of its implications or by ascertaining what these are—we find ourselves beset with serious difficulties; as the past history of psychology and its present controversies sufficiently shew. Just for these reasons, then—because a rough and ready characterization of psychology is easy, while any adequate determination of its standpoint and scope would be a tedious and arduous undertaking—this preliminary inquiry is often deliberately ignored even by writers of high repute. And yet the problem is one of central importance, at any rate to those who have any interest in philosophy. Epistemology and ethics, the theory of knowledge and the theory of conduct, rai questions which depend in large measure for their solution on t conclusions we reach concerning this. In the history of British thoug in particular the influence of the conception of psychology on met physical and ethical speculation is unusually striking. I shall therefo assume that such introductory discussion is not one that cultured an thoughtful persons will care to leave altogether aside.

We shall perhaps start best by means of a brief historical retr spect. It is by knowledge of others that the child advances to co sciousness of itself: it is aware of third persons, even of itself as o of these, before it realises its unique position as first person. An when at length this unique position is realised it is apt at first to unduly predominant, as we frequently notice in the so-called excessi or morbid self-consciousness of youth. A like order and one-sidedne is evident in the growth of psychology: it was first unduly 'objectiv and then unduly 'subjective': it is only now beginning to shew sig of maturity in a due balance of the two: the fundamental conceptio of the first period was *Life*, that of the second, *Mind*, that of the third *Experience*. To understand this last then we must consider the oth two in turn.

An intelligent person beginning to study [illegible] *novo* the broad fac which here concern us, unaided and [illegible]led by traditional current theories, would almost certainl[illegible] do, what according Tristram Shandy Locke did, write "a hi[illegible]ry book of what passes his own mind." He would in all probability be first impressed by th close connexion between the facts of mind and the facts of life whic he observed on all sides, by the close connexion, that is to say, of livin mind and living body. At any rate this was the case with primiti thought, as philology and anthropology amply prove. But it is needle to go back further than Aristotle, whose *De Anima* marks the birth Psychology as a separate science. Let us note then that Aristotle— in sharp contrast to Descartes, whom, rather than Locke, we ma regard as inaugurating the second period,—began his study of min from the side of body. He divided natural bodies into those that hav and those that have not, life. The former in all cases consisted organs mutually adapted to a specific end: they were, as we now sa organisms, or rather potential organisms, the indispensable physic bases of life. The soul on the other hand was that by which and f which this potentiality was realised and manifested. Body and so were then inseparable correlatives, like the matter and form in the co crete whole we call a seal. "To speak of the soul as feeling anger," he remarks, "is like speaking of the soul as weaving, or building a house. It is better not to say that the soul pities or learns or thinks, but rather that the man does so through his soul." What its function was to a particular organ—vision to the eye, for example—that the soul was to the organism as a whole: it was "the cause and principle of its life[1], "by which is meant," says Grote, "not an independent and pre-existing something that brings the body into existence, but an immanent or indwelling influence which sustains the unity and guides the functions of the organism[2]." Of souls Aristotle recognised an ascending series of kinds, falling into three chief classes—plant-souls, animal-souls, and human souls, each higher kind possessing all the functions of the lower in addition to its own. Now in the case of plants and animals—and of man, so far as he shares their characteristics—these functions could be inferred from the corresponding organs. Thus the souls of plants were nutritive and generative, those of animals were, besides, sensitive, appetitive, and—usually—locomotive. Plants did not need sensation, but all animals had to have the sense of touch (and taste, which is a sort of touch) in order to avoid obstacles and secure their appropriate nutriment. The other senses, however, as directed to higher ends than mere existence, belonged only to particular classes of animals[3].

So far Aristotle's point of view resembles that of the modern biologist. His conception of 'soul' has few of its modern associations, while it is closely related to the physiological conception of function. Like this it implies not only the organism—as vision, *e.g.*, implies the eye—but it implies also the environment, as actual vision implies light. Like this, again, it knows nothing of the dualism of life and mind: mental processes have an organic basis as truly as vegetation, and when they exist they are simply higher functions of the same soul that realises these. But Aristotle differs from most modern biologists[4], first, in making the conception of final cause fundamental. "All natural bodies," he says, "are instruments of the soul: and just as it is with the bodies of animals so it is also with those of plants, all being there simply for the sake of the soul." The self-preservation and well-being of the living individual and its kind are the end of all organic processes, that is of all interaction between the organism and its environment. Aristotle differs again from most biologists now in regarding the soul as the 'primary source of local movement,' that is to say as the directive principle in this interaction[1]. For these reasons it would be inexact—in spite of the resemblance—to describe Aristotle's *De Anima* as biological. For the present we shall do better to call it objective psychology: it contemplates psychical facts inferentially from without, rather than introspectively from within. As a result of this attitude organic life and psychical life may appear at first to be too much identified. But from the opposite standpoint, the exclusively subjective, to which we must presently turn, perhaps we may find their complete separation to be equally extreme.

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[1] *De Anima*, II. iv. §§ 8 ff.

[2] *Aristotle*, p. 460.

[3] *De Anima*, III. xii. § 8.

[4] The rising school of Neo-vitalists is, however, an exception.

When however we reach Aristotle's treatment of the human soul as intellectual, we come upon a certain discontinuity. For Aristotle found no organ of intellect, and even speaks of intellect (νοῦς) as 'separate, impassive, and uncompounded [with material conditions][2].' But if intellect have no bodily organ, in what sense is the soul of man the actualization of his body, and how can Aristotle compare the unity of soul and body here to that of the wax and the figure impressed upon it, or to that of the axe and the material of which it is made? Before attempting to deal with this difficulty we must take account of two very different senses in which Aristotle speaks of reason or intellect. His doctrine of active intellect (νοῦς ποιητικός), the first of these, is rather theological than psychological: it is in the main his philosophical version of the widely held belief of man's participation in the divine. This creative reason comes from without, it is impersonal, immortal, and eternal: it is comparable to the sunlight by which we alike see and through which alone things become visible. It is only this phase of reason that 'is separate, impassive, and uncompounded [with material conditions].' The receptive or passive reason (νοῦς παθητικός) on the other hand is a personal endowment and varies greatly from individual to individual: this 'is perishable and can really think nothing apart from the creative reason[3].' Here at any rate we should find no breach of *psychological* continuity if we were to follow in detail Aristotle's exposition of this individual reason. The popular summary of it is perhaps sufficiently exact: *Nihil est in intellectu quod non prius fuerit in sensu;* through sensation, phantasy, memory we advance to recollection, conception, and intellection[4]. The higher processes presuppose the lower, and these—sensation, imagination and memory (or retentiveness)—depend directly on the organism. And but for certain physiological errors into which he fell[1] Aristotle would doubtless have found the connexion between the organism and the soul as intellectual more direct and more definite than he supposed; though, even as it was, he made the intellectual part of soul wholly dependent on the organism. For in man, the active intellect operates only under the stimulus, as it were, of the passive, and this again receives all its material from the senses. In any case it was inevitable that in advancing to these higher functions he should approach nearer to the subjective standpoint: even with our present knowledge we could learn little or nothing about intellectual processes if we attempted to begin by studying the brain. There is still however a wide difference between Aristotle's exposition of them and the exposition of an introspective psychologist. It is not thinking as a process in the individual mind so much as thought as a universal product that Aristotle considers, and when—upon occasion—the individual as distinct from the universal aspect of thought is foremost with him, then biological or physical analogies are apt to obtrude. "The plant assimilates the material in a material manner, sense assimilates the material in an immaterial manner and thought assimilates the immaterial in an immaterial manner[2]." What we miss in Aristotle is a clear recognition of what we now call consciousness as the central feature of all psychical facts. Regarding these facts as he does from the outside rather than from within, from the circumference rather than from the centre, he fails to find an adequate unity for the diverse functions which he describes: he has to rest content with the biological conception of an organism, into which, however, he infuses a strong teleological colouring.

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[1] *De Anima*, II. iv. § 6. [2] *De Anima*, III. v. § 2. [3] *De Anima*, III. v. § 2.

[4] No doubt Aristotle would concur in Leibniz's addition of *nisi intellectus ipse*, meaning thereby the universal and creative reason that illumines and interprets the data

When we pass to the psychology of Descartes we are at the opposite extreme. The connexion of body and mind, the corner-stone of Aristotle's construction, was the chief stumbling-block in the way of Descartes' advance, and has remained as a perplexing problem even to

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of experience. But this, as soul, is a metaphysical tenet which carries him altogether beyond psychological bounds. But as we may see later (cf. below, p. 18) it is possible to advance much further than Aristotle did without having recourse to philosophical speculations.

[1] Unlike Plato, Aristotle held the heart, not the brain, to be the central organ or seat of the soul. The fact that the hemispheres were insensitive to stimulation confirmed him in this view.

[2] Bäumker, *Des Aristoteles Lehre u.s.w.*, quoted by Wallace, *Aristotle's Psychology*, p. lvi.

our own day. The hazy materialism into which the Aristotelian psychology had developed in mediaeval times Descartes banished once for all by the new definitions which he gave of matter and mind. Both were substances and therefore essentially distinct: the essence of matter was extension or the occupation of space, that of mind was consciousness; and between these there was no common term and no natural connexion.

Cogito, ergo sum, Descartes began: 'I think, therefore I am.' This was for him the primal certainty, the starting-point alike of his philosophy and of his psychology. "By the word thought (*cogitatio*)," he tells us, "I understand all that which so takes place in us that we of ourselves are immediately conscious of it; and accordingly not only to understand, to will, to imagine, but even to perceive (*sentire, sentir*) are here the same as to think (*cogitare, penser*). For if I say, I see, or, I walk, therefore I am; and if I understand by vision or walking the act of my eyes or of my limbs, which is the work of the body, the conclusion is not absolutely certain...: but if I mean the sensation itself, or consciousness of seeing or walking, the knowledge is manifestly certain, because it is then referred to the mind, which alone perceives or is conscious that it sees or walks[1]."

Here then we are unmistakeably inside the circle which Aristotle regarded mainly from without, and the central unity which we missed in his exposition is now clearly indicated. Subjective psychology deals with whatever we are *immediately* conscious of as something taking place *within* us: with the biological aspects, the physical occasions or the logical validity of this something it has no concern. All that it essentially implies is a conscious individual (a *res cogitans*) and the various actions and passions of which it is conscious—'its diverse modes of thinking,' or 'the contents of its consciousness,' as some would say. So far from a body being necessary to the existence of a conscious mind, as Aristotle from his objective standpoint assumed—and naturally, for it was with the living body that he began—the distinctness and independence of the two is, Descartes maintained, at once evident so soon as we reflect on the nature of consciousness. We then "perceive clearly that neither extension nor figure nor local motion......pertains to our nature, and nothing save thought alone:

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[1] *Principles of Philosophy*, pt. I. § 9. In equating Descartes' *cogitatio* to the modern 'consciousness,' which is on the whole the best rendering, we must not forget the predominantly cognitive implication which it, even more than its present equivalent, always retains.

it then becomes plain that I am not the assemblage of members called the human body; I am not a thin and penetrating air diffused through all these members, or wind, or flame, or vapour, or breath; for *the notion we have of our mind precedes that of any corporeal thing*, and is more certain, seeing we still doubt whether there is any body in existence, while we already perceive that we think[1]."

This restriction of psychology to the immediate facts of consciousness as these exist for the conscious subject was a great advance on the confusion of psychology with biology which characterised the Aristotelian and scholastic doctrines. As a result the science made more progress in two centuries than it had made in two millenaries before. But as so often happens, the reaction—as we have already hinted—was excessive; this we shall see if we examine the Cartesian dualism a little further. Whereas Aristotle on the whole kept to facts, Descartes trusted to analytic distinctions. Aristotle found mind and body invariably connected, and therefore he regarded them as essentially inseparable. Descartes could conceive mind without body and body without mind, hence his conclusion that they were actually independent and could exist apart. But what sort of mind was it that Descartes thus conceived? Broadly speaking it was the human soul of Aristotle less the senses, memory and imagination which—on Aristotle's view—man shared with the lower animals and required as indispensable conditions of his own activity. The thought that properly belonged to this soul excluded everything we now call empirical: hence the dualism of reason and experience that reappeared in modern philosophy. This *res cogitans* of Descartes then could only be occupied with eternal truths or 'innate ideas' and such other ideas as it might itself frame from these: 'adventitious ideas' it would not have at all. But even at this point a little reflexion will convince us that—as was afterwards shewn by Kant—such a consciousness as this Cartesian *cogitatio* is not really conceivable. It lacks individuality and it lacks concreteness. For the environment and the intercourse with other selves—on which any consciousness of self depends—are so far wanting. In other words, as yet the conditions of actual experience are incomplete.

Let us now turn for a moment to material substance, the second term in the Cartesian dualism. As sensations were not to be attributed to mind as *res cogitans*, so here sensible qualities are not to be attributed to matter as *res extensa.* Only so far as matter was "the obje speculative geometry" was its nature intelligible, and for this k ledge sensory experience was superfluous; nay, worse—it was leading. Descartes' *res extensa* was thus even more than hi *cogitans* a merely analytical conception. In the latter case the vidual *Cogito* was at least a certainty, though one which his conce of mind-substance did not explain, but here he only contrive differentiate his matter-substance into a plurality of material thin a series of glaring subreptions and incongruities. In both case fault lay in his rationalistic attempt to derive the concrete fac experience from purely abstract notions. Dynamical conceptions, as those of mass and force, which only experience could warrant, smuggled without clear definition or derivation into a physics professed to be "nothing but geometry." Yet in spite of these i defects the impetus that Descartes gave to Natural Philosophy even greater than that which we have allowed is owed to hi Mental Philosophy, and the achievement here again was due t famous method. As he cleared the conception of consciousness of materialistic implications so he cleared that of matter of the ani involved in the mediaeval notions of occult qualities such as the na gravitation of earth, the natural levitation of air, Nature's abho of a vacuum, and the like. But the details of his Natural Philos do not now concern us: it is enough to recognise that in it mecha notions were supreme throughout. An organism accordingly wa Descartes simply a mechanism, an integral part of the one mechanism called the external world. So far then from conne biology with psychology, as Aristotle had done, Descartes red biology to physics.

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[1] *Principles*, I. 8, and *Meditation*, II. (Veitch's ed. p. 108).

And now what of the connexion of body and mind? We note of all that Descartes inverted the Aristotelian position that int presupposes sense[1]: according to him sense presupposed inte "I find in myself," he says, "the faculties of imagination and sens (*sentir*), without which I can indeed clearly and distinctly con myself entire, but not reciprocally them without myself, that is t without an intelligent substance in which they reside, for...in formal concept, they involve some sort of intellection[2]." Fi further "not merely that brutes have less reason than man, but they have none at all[1]," he concluded that they were nothing but automatic machines, entirely comparable—save for their greater complexity—to the contrivances of a skilful clockmaker, needing, as he expressly said, "neither a vegetative soul, nor a sensitive soul." Even the human body, physically regarded, was only such a machine.

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[1] So far, that is, as Aristotle did assume it.

[2] *Meditation*, VI., Veitch, p. 157, also p. 152.

Nevertheless the relation of man's soul to his body was not comparable to that of a pilot in a seaworthy boat: after all the two become a single substantial unity:—"Me non tantum adesse meo corpori, ut nauta adest navigio, sed illi arctissime esse conjunctum et quasi permixtum, adeo, ut *unum quid* cum illo componam[2]." But how was such substantial unity possible? To answer this question reason was helpless and even the criterion, on which Descartes' whole method of philosophizing was founded, proved at fault. This he frankly owned. "To me it seems impossible," he writes, "that the human mind should distinctly and at the same time conceive the distinctness of body and soul and likewise their union; for so to do, it must conceive them as a single thing while yet conceiving them as two, which is self-repugnant[3]." Yet Descartes never denied that the unity was at any rate a fact, however inexplicable, and a fact that rendered human experience possible. Nay, strange to say and in spite of his general rejection of final causes, Descartes concludes his *Meditations* by pointing out—in the style of a Bridgewater treatise—the mutual adaptability of body and mind manifested in our daily experiences. He concludes by laying down the maxim:—"I ought not in the least degree to doubt of the truth of those presentations ['of my body surrounded by many other bodies'], if, after having called together all my senses, my memory, and my understanding for the purpose of examining them, no deliverance is given by any one of these faculties which is repugnant to that of any other[4]." But on the senses exclusively, as Descartes allowed, we depend for the knowledge that material things *actually* exist; and it is equally certain—though this he did not explicitly allow—that but for memory we should be without that knowledge of our own existence, from which he started. Both sensation and memory, however, belong to man only as a rational animal, not to man conceived as *intellectus purus*. In other words, intellect alone is not the source of our real experience. But it is the source of the conceptions of *res cogitans* and *res extensa* as disparate and mutually independent substances, the conceptions, that is to say, on which the Cartesian dualism is founded. For this dualism, then, our concrete human nature is not merely a glaring exception—since all other spirits are assumed to be incorporeal and all other organisms merely machines;—it is not merely a knot that an omnipotent Deity might tie: it is a veritable *Unding*, a contradiction. Since, however, this human nature is a fact, it suffices—even as a negative instance—to render that dualism untenable, and we only need to begin where Descartes ends to be clear of it. For in the end, as we have just seen, he has to admit that it is not true of human nature, and he fails to find it in human experience. Here, as he points out, our internal sensations make us aware of what we need for the preservation of health, and our external perceptions enable us clearly and distinctly to know which among surrounding objects are beneficial and which are hurtful to us in so far as we are composed of body and mind; here memory enables us to connect together the whole course of our waking life; and here judgment enables us to discriminate practically between what is true and what is false, so that "all the doubts of those bygone days are to be rejected as hyperbolical and ridiculous."

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[1] *Discourse on Method*, pt. v., Veitch, p. 57.

[2] *Meditation*, VI., Veitch, p. 160. But in view of the importance of this passage it seems worth while to give the original.

[3] Letter to the Princess Elizabeth, June, 1643.

[4] *Meditation*, VI., Veitch, p. 168.

Had Descartes started, as he ought to have done, from this experience, and reflected seriously on all that it involved, he might have realised that his conception of mind alone was not adequate to cover it. Beginning from the objective side—that of organism or environment—Aristotle saw that soul was necessary in order that the organism should actually have life. Descartes, who began from the subjective side—that of consciousness—ought in like manner to have seen that the organism and the environment were necessary in order that the conscious subject should actually have experience. They are the objective factor, it is only the subjective. If the one factor was entitled to be called *res cogitans*, the other had an equal title to be called *res cogitata*, and the fact of *cogitatio*, so far as it proved the reality of either, proved the reality of both. But Descartes failed to seize this duality. It is true that he admitted, and admitted in so many words, that in human nature the *res cogitans* is not a *res completa*[1]. But, after all, this admission was made from the biological or psychophysical standpoint, the standpoint of Aristotle, not from the psychological standpoint, to which Descartes had himself attained. He therefore still held fast to his dualism. The immediate objects, even of sensible experience, he maintained were only *modes* of consciousness, changes "that take place in us."

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[1] Reply to Arnauld's objections to the *Meditations*.

But how is this position to be made consistent with Descartes' belief that his own body was surrounded by many other bodies, and so forth? Were those presentations of his own body and other bodies but modes of himself as *res cogitans*? If not, then his experience was not confined to modes of his own consciousness. If so, it was so confined and therefore cut off altogether from body as a *res extensa*: the dualism of mind and body is justified indeed, but only at the price of making all experience of the latter impossible. Out of this second *impasse* Descartes only escaped as he escaped from the first—by appealing to the Deity: only the Divine omnipotence could combine body and mind in human nature, and only the Divine veracity could guarantee the reality of the material world in human experience. These two problems—the relation of body and mind, the reality of external perception—have continued to vex philosophic thinkers from Descartes' day to our own, nor will they cease to trouble us till dualism is laid to rest.

On these grounds alone we should be amply justified in rejecting *in limine* the perfunctory definition of psychology—etymology notwithstanding—as the science of mind, over against which there stands a totally distinct science of matter (which might have been called hylology). It will repay us, however, to continue our historical survey a little further, so as to note the main feature in the transition to a third definition of psychology as the science of individual experience, in which the respective merits both of the Aristotelian and the Cartesian doctrines are retained, and their defects redressed. The chief merit of the second of these lies, as already said, in its subjective, *i.e.* individualistic standpoint: this has not been, and is not likely to be, abandoned. The defects consist partly in its metaphysical, we might even say, its theological assumptions, and partly, as a consequence of these, in its imperfect analysis of—the predominantly 'intellectualistic' character it assigns to—individual experience. Though the dogmatic assumptions of Descartes' mental philosophy were seriously discredited by the empirical psychology which Locke began, and a long line of British workers carried forward, yet that philosophy continued to flourish on the Continent, attaining its zenith in the *Psychologia rationalis* of Wolff, wherein the simplicity, immateriality and immortality of the soul were evolved out of the bare conception of consciousness. But such *a priori* demonstrations of the nature of mind were at length rudely shaken, along with the rest of metaphysical dogmatism, by the *Critique* of Kant, who maintained the emptiness of all conceptions save as they derive their 'content' from experience, and the invalidity of all conceptions when extended beyond it. For us there were no *noumena* or thought-given realities: all our knowledge was confined to *phenomena* or sense-given realities. To experience the duality of subject and object was essential, and its two factors in isolation were not *res completae* but purely problematic conceptions, about which there might be faith or speculation, but certainly not knowledge. In whatever way our practical interest in such problems as that of immortality may be met, they have, at any rate since Kant's day, ceased to be regarded as psychological problems[1], and psychology has now become entirely an empirical science, divested alike of theological and of metaphysical assumptions. The recognition of the inseparability of subject and object in experience too, which was a cardinal doctrine with Kant, has helped to bring the mind theory into line with the life theory; but in place of the life of body, organic life, we have now the life of mind, psychical life. But mind here properly denotes the subject of experience, the Ego—as we sometimes say—in contradistinction to the Non-Ego or object of experience; and mental life is tantamount to experience as it is for this subject. It is with this mental life that Subjective Psychology, as contrasted with the Objective Psychology of Aristotle, is primarily concerned[2].

But Locke and his successors, Kant included, were still hampered by the defective analysis of the facts of mental life, which they took over from Descartes, while rejecting more or less completely his metaphysical assumptions. That analysis, it has just been said, was unduly intellectualistic: in other words, as Descartes conceived mind as essentially intellectual, so he regarded experience as fundamentally cognitive. The only experience he recognised was self-conscious experience; and in this he tended first to identify the experience with the self-consciousness, the whole with the part, and next to identify the cognitions of self-consciousness with the facts cognised. Each of these twin errors we must examine in turn.

In external perception the mind, Descartes conceived, "turned towards the body," but in self-consciousness "it turned in some way upon itself." In keeping with this Locke distinguishes sensation and reflexion as the two sources of simple ideas, the one of the ideas of the sensible qualities of external objects, the other of the ideas of the mind's own operations. Reflexion, though not actually a sense, is yet, he says, "very like one, and might properly enough be called *internal sense*[1]." And Kant proceeded without misgiving so to regard it and placed external sense and internal sense on a par as distinct but co-ordinate sources of experience, the one of the experience of physical phenomena, the other of the experience of psychical phenomena. So we get a new dualism of phenomena, which serves to keep the old dualism of substances in countenance[2]; and with it we get also a new definition of psychology which is scarcely better than the old. Psychology becomes the science of internal experience as observed through the inner sense, and so is sharply contrasted, though otherwise co-ordinate, with the sciences of external experience, which treat of t[illegible] objects observed through the outer senses. One step more and the subject and the object of our immediate experience seem again to fall completely apart. This step was taken, for example, by Bain, who distinguishes object-experience from subject-experience, and confines psychology to the latter. He further refers to these as two worlds, "the one circumscribed by one property, extension," the other definable "negatively by a single fact, the absence of extension[3]." But it is certain that immediate experience is never thus sundered, and obvious, therefore, that in all this there is some confusion which we must endeavour to clear up.

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[1] For Kant himself immortality was a postulate of the practical reason.

[2] Cf. on this distinction, H. Spencer's *Principles of Psychology*, pt. I., ch. VII.

We may note first of all that the phrase 'internal sense' is a complete misnomer, save where reference is intended solely to what is internal to the organism. But here 'internal' is meant to distinguish what occurs 'in the mind' from what occurs out of the body, and involves a correlation of 'in' and 'not in,' *i.e.* 'out of,' which is as absurd as contrasting what occurs in a given day with what occurs outside a given door. And as to an internal *sense*

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[1] *Essay*, II. i. 4.

[2] Thus we find Hamilton saying: "Mind and matter, as known and knowable, are only two different series of phenomena or qualities; mind and matter, as unknown and unknowable, are the two substances in which these two different series of qualities are supposed to inhere. The existence of an unknown substance is only an inference we are compelled to make, from the existence of known phenomena; and the distinction of two substances is only inferred from the seeming incompatibility of the two series of phe[illegible]omena to coinhere in one." *Lectures on Metaphysics*, vol. i., p. 138.

Mental Science, pp. 1 f.

—even if it were allowable to speak with Locke of sensory "impressions of *objects* extrinsical to the mind," what would be the meaning of sensory impressions from "powers intrinsical and proper to [the *subject*] itself"? The physiologist who recognises organs and 'centres' of the outer sense knows nothing of any such in the case of this supposed 'inner sense.' Locke bids us "follow a child from its birth and observe the alterations that time makes," and he then himself briefly describes the child's gradual advance till "in time it comes to reflect on its own operations about the ideas got by sensation." But when this stage is reached Locke does not suppose that the child passively receives impressions differing from all previous ones, as the sensations of colour for one couched differ from all his preceding sensations. In the earlier stage the child was conscious, but not self-conscious: "the constant solicitation of the senses," as Locke says, "[illegible]hen employed and directed [it] in looking abroad." But when at length "it turns its view inward upon itself, and observes its own actions about those ideas it has," it becomes self-conscious; but it does not thereby acquire a new mode of what Kant called sensibility, comparable to the addition of a sixth sense to the five it had before. On the contrary it is only intellectually active "*about the ideas it* [*already*] *has*[1]." Beforehand it could not hear that it tasted, or taste that it heard; nor can it now, for the external senses are severally distinct. But beforehand when it tasted it was not conscious of tasting, when it heard it was not conscious of hearing, as it may be now. In short, on the new level of self-consciousness the objects of the external senses are recognised as having new relations by the miscalled 'internal sense': in other words, the so-called object-experience seems clearly implicated in the so-called subject-experience. How, then, can psychology be confined to the latter?

Nevertheless, must not psychology be so confined if it is the science of individual experience: otherwise wherein lies the one merit accorded to Descartes of making its subjective standpoint once for all clear? Moreover, if it is to embrace also the experiences attained from the objective standpoint, will not the whole of knowledge fall within its domain? Questions such as these, which will naturally occur at this point, lead us at once to the main source of the confusion we are discussing.

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[1] Thereby indeed it acquires other ideas, but these are not sensory and cannot wit[illegible] any propriety be called *impressions* of reflexion, as they were by Hume, for example.

What we have first to ascertain is whether the disjunction sug- ested is complete. Must the experience with which psychology is oncerned be either confined to what can be known about the subject f experience, or be extended to include all that is known about the bjects of experience? In other words, is the subject the only factor mplicated when we occupy the subjective standpoint, and the object he only factor implicated when we occupy the objective, much as we ight say that sound alone concerns us when we study acoustics, and ght alone when we study optics? Certainly if we were all deaf the rmer science would be non-existent, and the latter if we were all lind. But we have just seen that this analogy does not apply to the istinction of internal phenomena, the facts of the so-called inner sense, nd external phenomena, the facts of the external senses. They are ot co-ordinate and they *are* mutually implicated. Of this the term henomenon, applied to both, is evidence; for when a phenomenon or ppearance is actual, there must be someone to whom it appears, for hom it is a fact; and nobody will maintain that internal phenomena re exclusively perceived by one subject, and external exclusively by nother. Thus we find Bain, who began by distinguishing subject-xperience from object-experience, presently admitting that "object-xperience is also *in a sense* mental[1]." But in what sense is it mental, hat is to say, pertaining to psychology; and in what sense, not? This s the question that ought to clear up the confusion, which Bain was ontent to leave alone. We are agreed that psychology deals with the bjective standpoint of individual experience, but we find that in this xperience both subject and object are factors: we have to ask, then, ow this standpoint differs from what we call the objective standpoint, n which, apparently, the subject is not a factor. And we can answer t once: the one is the standpoint of conscious Life—or more fully, e standpoint of the living subject in intercourse with his special nvironment; the other is the standpoint of science in which the aracteristics of individual environments are in general ignored. But there were really no subject implied in the standpoint of science, how uld we speak of science as concerned with object-*experience*, and as aling with actual phenomena; and what would be the meaning of standpoint which was altogether unoccupied? But the truth is t that—for science—no subject, but only that no single subject, is plied, by whom—as for psychology—the experience is conditioned. eling and conation, which are inseparable from living experience, are therefore omitted. Science is concerned with knowledge only, knowledge as it is for all; and again with knowledge only as the product c many co-operating minds, not as a process in one.

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[1] *Mental Science*, p. 2.

The failure of the pre-Kantian thinkers to apprehend the bearin upon psychology of a distinction in itself so clear, was due in the firs place to their neglect of comparative psychology and the consequen restriction of the science to the data of self-consciousness which th neglect entailed. They recognised indeed, as we have seen, tha Aristotle—and still more, Descartes—did, the discontinuity that th possession of self-consciousness and reason placed between man an the lower animals. But they did not realise that both reflexion an reasoning are the result of social intercourse, the gradual developmen of which has placed such a gulf between man and brute. Assumin that each man by himself is rational, and not that humanity ha achieved rationality, they then proceeded to confound psychology wit that division of philosophy which is now called epistemology, or th theory of knowledge. In fact, it was mainly for the sake of epistemc logical problems that they were led to take up psychological investigation at all[1]. It was reserved for Kant first to discern the fundamenta difference between the two inquiries, thanks, however, to the philosophical deadlock into which his predecessors were led by confusin them. But we can all now see that 'subjective' and 'objective' hav different meanings in psychology and in epistemology. In the latte 'objective,' we might say, means so much of experience as is commo property, and 'subjective' so much as is private property: in th former 'subjective' refers to the owner and 'objective' to the propert that he owns. But science regarded what is common property as it were not property at all, and psychology assumed private ownershi to be the only ownership. Again, since the 'subjective objects' psychology were not found among the 'objective objects' of scienc they were regarded as only copies or symbols of these originals, whic science placed somehow within the man's head and psychology foun within his consciousness. So the result was reached: a subject witho real objects, and real objects without an assignable subject, an inextended subject-world and an extended object-world without any satisfactory account of their connexion. The Cartesian dualism still lingered on. This was the *impasse* that led Kant to expose the 'transcendental realism' of the latter world, and Reid to protest against the 'subjective idealism' of the former. What Reid meant to say was: in perception we are not conscious of ideas in us, but we affirm objects present to us. What Kant said was, the objects of science are only objects for and through our common experience; they are not things in themselves and apart. Combined, these statements amount to a recognition of the duality of subject and object throughout all experience, individual as well as universal. But still the psychological alysis of Kant and Reid was inadequate to do justice to this duality ndividual experience as one of Ego and Non-Ego, Self and Other. [illegible] point brings us to the second of the twin errors just now sed, the tendency to treat the facts cognised by the self-conscious [illegible] in reflexion as being themselves cognitions.

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[1] This was avowedly the case with Locke: his famous *Essay* only professed to d with the human understanding; Hume's mature work had the same aim and an alm identical title; and Berkeley called his one systematic treatise, *A Treatise concerning Principles of Human Knowledge*. Even writers as recent as Hamilton and Mansel o treat of psychology under the name of Metaphysics. Indeed until comparatively la the interest in psychical facts rarely extended further than seemed required by s problems as those concerning the criteria of knowledge, the grounds of moral respo bility or the existence of a life beyond.

[illegible] in one form or other is an inveterate error: I have formerly d it as "a confusion between the standpoint of a given xperience and the standpoint of its exposition," and as "one to which o other science is liable except psychology and the sciences dependent on it." The latter is perhaps an over-statement; nevertheless rofessor James afterwards gave this error the name of 'psychologist's llacy,' and his name for it is now commonly adopted[1]. As a conquence of this fallacy the pure feelings of pleasure and pain, for ample, which are entirely subjective—in the psychological and not rely in the epistemological sense—were described as psychologically jective, and classed among sensations or perceptions, because we ne to have ideas of them when we attain to the standpoint of social ercourse and self-consciousness. And again because at this level [illegible] general connexion was discernible between pleasure and increased tality on the one hand, and between pain and diminished vitality n the other, the feelings themselves were identified with the consciousness of some perfection or imperfection in ourselves, and finally defined [illegible] Wolff as "the intuitive cognition of any perfection or imperfection ver, real or apparent[2]." This failure to realise the purely sub-

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f. J. Ward, "A General Analysis of Mind," article in the *Journal of Speculative* *sophy*, 1882; W. James, *Principles of Psychology*, 1890, vol. i., p. 195.

Cf. Descartes, *Letter to the Princess Elizabeth*, Sept. 1645; Wolff, *Psychologia* *ica*, §§ 511, 518. It is a nice question how far this view is justly attributable to rtes, notwithstanding the unanimity that so far prevails on the point.

jective and unique character of feeling is common to all our earlier British psychologists from Locke to Reid. It was first corrected by Tetens, who insisted on what is called the tripartite division of faculties into cognitive, affective, and conative; but this classification, now almost universally accepted, only obtained general recognition through the advocacy of Kant, who was a pupil of Tetens. As a further consequence of their intellectualistic bias, the earlier psychologists strangely neglected the important *rôle* that bodily movements sustain in every stage of experience—a fact that even Aristotle had not failed to recognise. Not till the beginning of the last century[1], and then thanks mainly to the physiologists, were these movements seen to be not only an indispensable factor in every act of perception—as evidenced in listening looking, sniffing, tactually exploring—and a chief source of our knowled of the primary properties of things—extension and resistance—bu be also in the various phases of 'reflex,' sensori-motor and ideo- action, so many steps in the development of volition. But th alone was duly taken into account by psychologists who looked conscious reflexion for all their data, and since such mature vo at any rate normally, determined by reasons, the so-called activ were regarded as throughout secondary to the so-called intellectual.

But a decided reaction against intellectualism, which first set about a century ago among philosophers[2], has since been great extended and confirmed by the ascendency of evolutionary ideas, an the consequent growth of genetic and comparative psychology. Th result is that in the present day psychologists are coming more an more generally to insist that not intellect but will, not cognition b conation, not sensitivity but activity, is the clue to a true und standing of the character and development of experience. A wing cherub—all head and no body—might suffice, as Schopenhauer s gested, for the purely contemplative experience of Descartes' *cogitans*, but the fact that the inlets to knowledge are primar subservient to the inlets to food and air, which they encircle, shews unmistakably that experience, as the psychologist deals with it, is primarily and pre-eminently practical. Obvious as this must appear to those who look at the facts of life in the light of the theory of evolution, yet it is a truth that was for the most part overlooked so long as psychology was studied mainly in its bearing on philosophical problems. But the notion of an independent realm of truth existing *sub specie aeternitatis* has literally no place within the purview of a psychology that knows its business. Here we find no such thing as mere cognition: the uninteresting is not known but ignored, and the interesting leads at once to response, and sooner or later to adjustment in the race, at all events. Success is then completed experience or pertness, and in general prepares the way for a new advance. So ar the true is the useful, and the criterion is not theoretical but practical. Looking broadly at the progress of life, as it ascends through the animal kingdom and onwards through the history of man, it seems safe to say that knowledge is always a means to ends, is never an end by itself—till at length it becomes interesting and satisfying in itself. Psychologically regarded, then, the sole function of perception and intellection is, it is contended, to guide action and subserve volition—more generally to promote self-conservation and betterment.

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[1] The part played by the so-called muscular sense in the appreciation of 'weight' 'resistance' was pointed out by certain Italian physicians as early as the 16th centu (see Hamilton's *Reid*, p. 867 note), but their views failed to gain attention and wer forgotten.

[2] It began with Kant's assertion of the primacy of the practical reason, which reiterated with new emphasis: the *objects* of theory were there solely for the sake projects of practice, the external world is nothing but a means for the attainment moral end. Schopenhauer's *The World as Will and Idea* (indicating by its mere tit inversion of the old order), has, despite its disjointed and 'romantic' specu exercised a profound influence by its forcible and detailed defence of this topic, th many who have adopted his arguments have not thought it proper to mention his n

For psychical life so regarded, 'experience' is the obvious term, and the term which in our ordinary affairs is the one usually applied. But in psychology the much less appropriate term 'consciousness' holds the field, and its manifold ambiguities are something of a scandal. It is continually confused with self-consciousness, which was its original meaning; and thereby the errors of intellectualism, which we have just discussed, are apt to be perpetuated and a part of experience mistaken for the whole. "Everybody knows what consciousness is," we are told, "for everybody is conscious." But this is only true when it becomes trivial: every experient is experient. A mouse, for example, feels and strives: feeling and striving are factors of its experience, but they are not objects of its knowledge. They may become such for a man, no doubt; but there is much, even in his experience, of which we should say that he is conscious no longer or not conscious as yet. For in ordinary language we tend to speak of being 'conscious of' only what we specially attend to: in this sense the adept is no longer conscious of the painstaking efforts by which he first acquired his skill, and the tyro is not yet conscious of the differences to which, as a connoisseur, he will come to attend. In psychology, however, consciousness is

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regarded as admitting of indefinite gradations. Indeed this is often given as "its capital and pervading idea....Consciousness is co-extensive with mental life" in so far as "that life is considered to rise or to fall in degree[1]." Varieties of intensity are certainly characteristic both of the psychical and of the physical, but this fact alone would not serve to define them, even if—instead of being highly complex—they were merely simple processes, nor would it alone enable us to distinguish the one from the other. But then we hear not only of degrees of consciousness, but also of operations of consciousness, states of consciousness, contents of consciousness and form of consciousness. As instances of operations, perceiving, remembering, comparing, desiring, resolving, and the like would probably be cited. But, though it does not strike us as strange to speak of consciousness of perception or of desire—since for a self-conscious subject such reflective cognition is possible—it does seem forced to speak of consciousness remembering or desiring; for the self-conscious subject does not say: My consciousness remembers or desires, but, I do so. If, then, it is the subject of experience that is active, why should activity be attributed to consciousness, which after all is but an abstract term; not a conscious being, but the state of being conscious, which surely implies a conscious being?

The answer to this question is to be found not in the facts of experience but in the history of psychological theories concerning them. It is to be found, that is to say, in the reaction against the Cartesian doctrine, that experience is nothing but modes of a *res cogitans*. The conscious substance, it was held, lay beyond the pale of science, but the modes were supposed to remain within it; in other words, as we have already seen, the Cartesian analysis of mind was retained, though its philosophy of mind was rejected. This was a very *naïve* proceeding, for the so-called modes of consciousness are not conscious nor active, and without the explicit recognition of either subject or object are really unmeaning. Hence two alternatives were open. Some psychologists, having eliminated the subject of experience along with the substance, sought to correct their mistake by hypostatizing or personifying consciousness, and assigning to it the *rôle* of subject; these are the psychologists who talk freely of operations of consciousness and states of consciousness. Others have preferred to restore the missing reality from the object side; and they first resolve all the 'modes' into ideas or presentations, and then from such 'mind-stuff' and its interactions they proceed to build up experience in a quasi-mechanical, quasi-chemical fashion[1]. 'Content of consciousness' is the favourite phrase of psychologists of this class. Often they allow that such content of consciousness implies 'the form of consciousness,' implies, that is to say, a conscious subject, but they attempt, on methodological grounds, to justify the omission of all recognition of this which is only 'the *general* condition' of the content's existence and not a part of the content itself. Such a contention rests upon a complete misapprehension of the psychological standpoint. The start is apparently made from the natural or object sciences. The chief data of these sciences come to us by way of sight, or hearing, or touch, and as they come are regarded as entirely disparate. Now we can perfectly well conceive the problem to arise as to the means by which these severally incomparable groups of facts are to be related; and the solution to be found in eliminating the 'form' peculiar to each. So it might be supposed were obtained, first of all, Physical Optics, Physical Acoustics, &c., and finally, General Physics. Into the former there enters nothing of seeing or hearing, and into the latter nothing of sense at all. Now the 'contents of consciousness' or presentations may be regarded as a group of facts disparate with all beside. Why not proceed as before: eliminate the 'form of consciousness' or 'the inner sense' and then it may be possible to correlate these objects with the rest?—Not for this reason: the two standpoints are entirely different. The language the physicist uses is: there is this or that, *a*, *b*, *c*, or *d*; and though the psychologist in talking of him can say, *here is* such or such an experient, the psychologist cannot bring out the characteristics of his own standpoint by saying: there are such and such presentations or feelings or movements. To this end his statements must (and always do) take the form: *He, this experient, has* such and such presentations, feels thus or thus, and acts in this wise or in that. And this is 'the form of consciousness': to eliminate *it* is to ignore the concrete experience of the individual subject altogether, and to abolish what is characteristic of psychology. When its 'absolute presupposition' goes the content is no longer content of consciousness in the psychological sense.

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[1] So Bain, who gives this as the first of thirteen meanings of consciousness, a subject, which on account "of the subtleties and complications involved in it" he reserves for a closing dissertation, *Emotions and Will*, 3rd ed., p. 545. Again, Fleming: "The meaning of a word is sometimes best attained by means of the word opposed to it. *Unconsciousness*, that is, the want or absence of *consciousness*, denotes the suspension of all our faculties. Consciousness, then, is the state in which we are when all or any of our faculties are in exercise." *Vocabulary of Philosophy*, *sub voce*.

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[1] For this doctrine I have suggested the name of Presentationism: it is often called Sensationism or Associationism; the first because it regards sensations as the elements or atoms of which its 'contents of consciousness' ultimately consist: the second because the combination of these elements is supposed to be effected by a sort of 'cohesion' among those that are contiguous and an 'attraction' of those that are similar.

To deal adequately with experience we must combine what is positive in both these alternative views. The so-called operations and states of consciousness are not mere modes *in vacuo*: they imply an active and affective subject, and it can only conduce to clearness to make this fact as explicit as possible. The so-called contents of consciousness again are not necessarily actions or affections of the subject; and they are never objects *per se*; to be contents of consciousness they must be objects for the subject. The subject's activity, whether regarded as attention or as conation, and again the subject's feeling, have varying degrees of intensity or vivacity: its presentations or objects vary in like manner. But the two orders of intensities, though closely connected, are not to be identified. The form of consciousness cannot, then, be expressed by contrasting consciousness with unconsciousness in respect of intensity, nor by contrasting psychical phenomena with physical, the unextended with the extended, nor indeed by any single term which does not recognise the duality of subject and object. The one term that does recognise this duality most simply is experience. And experience we find is not merely nor primarily cognitive, and does not always attain, and is never entirely confined, to that joint-knowledge which the term consciousness vaguely indicates.

The most complex form of experience that we know is our own. We find experience assuming simpler and ever simpler forms as we pass backwards from man to the higher mammals, and from these to the lower mammals and birds, and thence to reptiles and fish. Long before we reach the end of the chain of animal life however it becomes a moot question whether there is any clear evidence of the presence of experience at all. Experience appears, that is to say, to be a comparatively late result of organic evolution, and human experience to be the summit of a long progressive series. Now this idea of gradual evolution which, under the title of 'the historical method,' claims "to have invaded and transformed all departments of thought," has certainly exerted a powerful influence upon modern psychology. It is the less surprising therefore—especially when we remember the defects of the older psychology—to find that the attempt is now frequently made to treat psychology wholly according to the historical, or as it is oftener called, the genetic, method. In biology such a procedure is possible; for the protozoan and man, the paragon of animals, are equally accessible as organisms. But the only *experience* immediately accessible is our own, and this—in spite of its complexity—is the first we know and the one we know best. Lower forms of experience, notwithstanding their greater simplicity, we know later and know less. Accordingly all attempts—regardless of this difference—to treat of human experience as merely the culmination of a long development, have so far been marked by serious defects. The start is avowedly physiological—from what is metaphorically described as 'organic behaviour,' meaning thereby such adaptability of organism to environment as seems to be determined solely and completely by the organism's structure, and from its automatic and invariable character to demand merely mechanical explanations. Later on, psychological conceptions are gradually introduced to eke out the shortcomings of the mechanical explanations when the spontaneity of the behaviour, and its varying adjustment to varying conditions, suggest that the machine is more or less under guidance.

So, as we advance, we pass as it were insensibly from biology proper to psychology proper, from the living protoplasm of the *Amoeba* to the living experience of man. We began with mechanism and we end with mind. But the psychology, when we reach it, is apt to be of the Presentationalist or Sensationalist type, since a psychology of this type can be most readily equated to the physiology from which the exposition set out. We have, that is to say, a 'physiological psychology' of the very worst sort; where physiological and psychological conceptions are for ever coquetting with each other, and where, as a result, unseemly hybrids are not infrequent[1]. If it be a sound maxim to proceed from the known to the unknown then Analytic Psychology, starting from human experience, must precede any attempt to treat of the genesis of experience as a whole, or to correlate psychology with physiology. And when psychology is regarded not simply as ancillary to philosophy or theology, but is studied throughout with scientific impartiality, there are happily facts in plenty within the range of human experience, though long overlooked as trivial or unimportant, which throw far more light on—say the problem of instinct—than biology alone could ever bring to bear. But in truth there is no question of a choice of methods: in every case physiological and comparative psychology must fall back on the facts and analogies of our own experience. The more carefully we study this, the more scrupulously exact we are in maintaining our standpoint fixed, the less likely we shall be to go astray.

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[1] Cf. *e.g.* Huxley's 'ideagenous molecules' as 'a physical basis of memory,' *Collected Essays*, I. p. 280.

ON BINOCULAR FLICKER AND THE CORRELATION OF ACTIVITY OF 'CORRESPONDING' RETINAL POINTS.

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SECTION I. Symmetrical Flicker.

A. Method employed. The rotating 'binocular' Lantern. B. Observations. Absence of evidence of summation or interference of flicker reactions, similar in degree and rate but varying from synchronism to different degrees of asynchronism, generated at 'corresponding' points.

SECTION II. Asymmetrical Flicker.

Evidence of interference of asymmetrical flicker reactions generated at 'corresponding' points.

SECTION III. Uniocular and Binocular Comparisons.

Talbot's law not applicable to 'corresponding' points. Rule of relation of binocular brightness to component uniocular brightnesses. Fechner's paradox. Prevalence of contours under Weber's law and under binocular summation compared.

SECTION IV. General Conclusions.

The physiological initial stages of the reaction generated in either component of a pair of 'corresponding retinal points' proceed without touching any of the apparatus of the twin point. Only after sensations initiated from the right and left 'points' have been elaborated, and have reached a dignity and definiteness rendering them well amenable to introspection, does interference between the reactions of the two (left and right) eye-systems occur. Since left and right end-results emerge pure, hybridisation cannot have mixed the early stages in their evolution. The binocular sensation attained seems combined from right and left uniocular sensations elaborated independently.

THE following experiments attempt inquiry into the influence exerted on the reactions of a 'retinal point' by certain forms of activity at the 'corresponding point' of the other retina.

By 'retinal point' is here understood the retino-cerebral apparatus engaged in elaborating a sensation in response to excitation of a unit of retinal area. In the inquiry much use has been made of flicker as a visual criterion. Almost without exception the experiments have confined themselves to the central (macular) region of the retina[1]; and the intensity of the physical illumination used has been well above the threshold for the light-adapted eye.

SECTION I. Symmetrical Flicker.

Ordinarily when the binocular gaze is directed upon an object intermittently illuminated, successive phases of illumination affect the orresponding areas of the two retinae synchronously. The question es, will the rate of repetition necessary for visual fusion of the uccessive light phases be altered if those phases fall upon corresponding retinal points not synchronously but alternately? To examine this question the following arrangement was devised.

A. *Method employed.*

A double sheet of thick milk-glass was observed by transmitted light given by a single-loop incandescent lamp, itself enclosed in a candle-shaped frosted glass. The lamp was fed at rather above its intended voltage, in order to give white quality of light, by accumulators unused during the experiment for any other purpose, and therefore supplying the lamp in constant measure. The lamp generally used was of 8 candle power, under a 100 volts. This lamp was set vertically in the axis of a rotating cylinder. This cylinder of turned brass was 78 mm. in diameter. In its side were cut three horizontal rows of rectangular windows tier above tier. The lamp though fixed in the axis of rotation of this revolving cylindrical screen was entirely free from all attachment to it. The milk-glass plate was fixed between the lamp and the inner face of the tiers of windows close to the latter.

Outside the moving cylindrical screen was a fixed semi-cylindrical screen concentric with the revolving one and just of width enough to allow the inner revolving one to turn within it freely. In the fixed cylindrical screen four circular holes were arranged so that two were centred on the same horizontal line, and of the other two one was centred as far above the left-hand hole of the just mentioned pair as the other was below the right-hand member of the pair. The horizontal distance between the centres of the right and left-hand holes was 9 mm. The diameter of each hole was 8 mm. The vertical distance between the centres of the holes was exactly the same as that between the centres of tiers of the revolving cylindrical

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[1] The experiments were the subject of a brief communication to the Royal Society's *Proceedings*, July, 1902, Vol. LXXI. pp. 71—76.

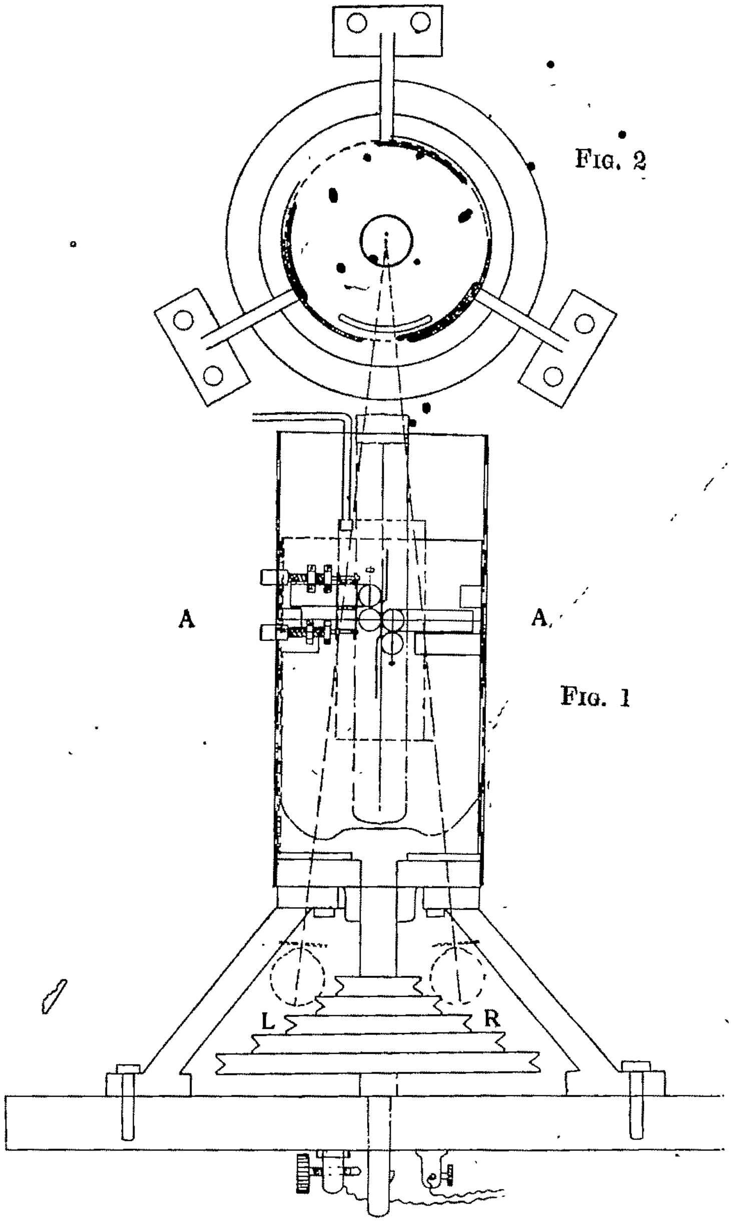

FIG. 2

FIG. 1

Rotating Lantern. Fig. 1. Elevation, seen from front. Fig. 2. Horizontal plan, through level of A—A, Fig. 1. Supports seen in perspective. The eyeballs, pupil screens, and convergent visual axes are indicated belonging to Fig. 2, but carried through Fig. 1. The plan of the lantern is given one-third actual size. Description in text.

screen, namely 11 mm. These four circular holes in the outer fixed cylindrical screen were, in the experiments, viewed from a distance such that when the line of visual direction of the right eye passed through the centre of the right hole it met at the axis of the cylindrical lantern the line of visual direction of the left eye, which latter line passed through the centre of the left-hand hole.

This being so the images of the lower left-hand hole and of the upper right-hand hole fused visually to singleness. They then appeared as the middle one of three arranged vertically one above another.

A black vertical thin screen set at right angles to the plane of the forehead was introduced between the eyes and the holes so as to screen from the left eye all view of the right-hand holes, and from the right eye all view of the left-hand holes. The distance of the eyes from the holes was in the observations on myself exactly 20 cm. For some observers a little less was used, their interpupillar distance being less than my own.

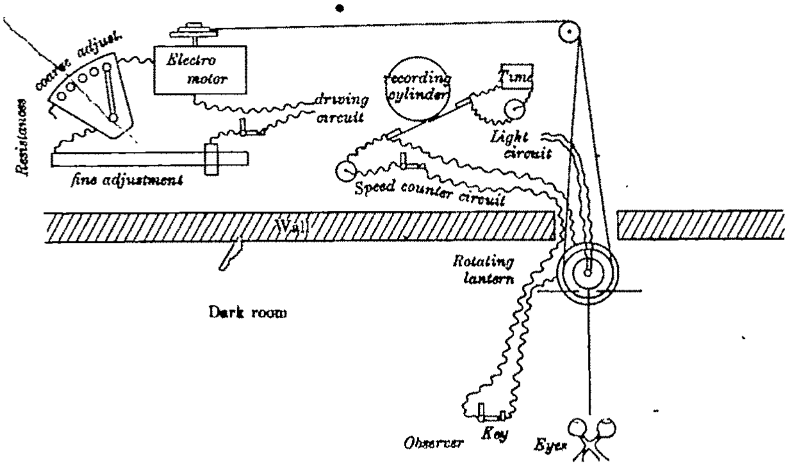

Diagram 1.

The spindle of the revolving cylindrical screen was furnished with a step-pulley. Thence a cord ran to a step-pulley fixed on the spindle of an electro-motor. The speed of revolution of this motor was controlled by a set of coil-resistances, which served as coarse adjustment, and by a fluid resistance in a trough 1 metre long, with a sliding electrode; this latter formed a fine adjustment. The speed of rotation of the cylindrical screen was recorded by marking the completion of each revolution of its spindle by an electro-magnetic signal writing on a travelling blackened surface. On the same surface the time was recorded by a writing clock marking fifths of seconds.

The inner revolving screen by its revolution opened and obscured alternately for equal periods the circular holes in the fixed outer screen. The inner screen with its three tiers of windows was made in three pieces, each containing one tier of the windows. The piece containing the middle tier of openings was jointed in such a in full binocular cooperation, owing to the concurrent convergence and accommodation. The slight discrepancy in amount between accommodation for a 21 cm. distance and convergence for a 23·5 cm. distance was corrected by making the prisms suitably convex, as in the Brewster stereoscope. Extinction and illumination of the images occurred *pari passu* in the two eyes, *i.e.* with like speed and in like direction. It could be synchronous or of any time-sequence desired. That the speed should be similar for the two was ensured by all the shutters being on the same spindle.

Each disc-shaped image would have on the retina a diameter of about 570 μ. That is, when foveal vision was directed upon it, the image would occupy a practically rod-free area containing about 2,800 cones. The direction of translation being the same for all the shutters bright images on the two retinae were, if the shutters were set for ultaneous right and left images, commenced on 'identical' points the two retinae, established progressively along 'identical' points, and finally extinguished in like manner progressively along 'identical' points. Or conversely if the shutters were set for accurately alternate right and left images the screening off began in one eye at a spot and moment identical with those at which the turning on of the image commenced in the other eye: so similarly it finished. With the speeds of revolution used for the observations, the time the shutter took to expose or occlude completely each bright disc varied between ·011″ and ·002″. Error that might have arisen on this score was avoided by the consensual direction of movement of the right- and left-hand shutters. Admitting an error of ·3 mm. in the cutting of the shutters, and this is an over-estimate, the maximum error in the turning on or off of the light from geometrically identical spots in the retinae is ·00042″ for the slow rates of revolution, and falls to ·000075″ in the high rates.

That the 'retinal points' to which the images were thus applied synchronously, or in desired sequence, were truly 'identical,' was certified, (1) by the paired physical images being seen single; (2) by the maximum disparation of the edges of the rotating shutters being about 7 μ on the retina, whereas 350 μ is about the vertical retinal disparation which limits binocular combination. Moreover, a contour travelling through a visual angle of 2° in $\frac{1}{30}''$, as in these observations, is not perceptible as a contour at all.

Difficulties due to change in pupil-width were excluded by the artificial pupils. Equality of brightness of illumination of the four milk-glass backed 8 mm. holes was obtained by making the straight-wire candle-shaped lamp of considerable, *i.e.* 12 cm., length, and fixing it accurately in the axis of the cylindrical screen. The rotating screen was blackened inside to minimise reflection. That the brightness of illumination of the four holes was really equal was ascertained at outset of each series of observations by finding the rate of revolution of the circular shutter required just to extinguish flicker at each of the four holes. If the rate of revolution was the same for all, the equalisation of the lighting of the four holes was considered to have been attained.

In a number of observations made before the arrangement of the apparatus took its final form, a single haploscopic image was presented to the visual field. The right and left components of this image could be either synchronously or alternately presented, according to the disposition of the apparatus, and it was easy to change the composition o the haploscopic image from one form to the other. But to effect change demanded a break of at least 5 seconds, and usually involv some disturbance of the observer by noise. This successive method comparison was at a disadvantage on these accounts; it suffered i facility and precision. The measurements it yielded were however broadly similar to those obtained by the method of more immediately successive comparison adopted finally.

In the latter, the apparatus for which is described in the small print above, two haploscopic images, one close above the other, were placed in the central field. The right and left components of each of these could be either synchronously or alternately compounded in each. The foveal gaze could be turned from one to the other of them when and as often as the observer desired, and in the fraction of a second by a slight, *i.e.* less than 3°, movement of the eyeballs. The comparison thus instituted was much more facile and sure.

The two images concurrently presented were so near together that it was easily possible while fixating the narrow dark interval between them to observe both and watch for appearance or disappearance of flicker in either. But the sensitiveness of even the median (central) region of the visual field is not everywhere the same as tested by perception of flicker. It was preferable to keep fixating the two images alternately, first one, then the other, and thus to watch for the earliest appearance (or disappearance) of flicker in either, using for each the macular retina itself.

An image would fail to flicker when received on the fovea that would distinctly flicker when its image fell just outside the fovea. Thus the disc image that just did not perceptibly flicker when its

B. *Observations.*

With the apparatus thus arranged various binocular combinations could be investigated and compared either one with another or with uniocular images.

As shown above the apparatus allowed of similar images being thrown on strictly and fully conjugate points of the two retinae, either synchronously right and left or alternately right and left, with a time accuracy not less than ·00042″ for the slowest rates of intermission, and not less than ·000075″ for the highest. The first comparison made (Experiment I.) was that indicated above at the outset, namely, to observe if there were any difference between the rates of intermittence for just perceptible flicker in two binocular images, one made with synchrono right and left illuminations, the other with alternate right and illuminations. This arrangement is expressed graphically b accompanying diagram.

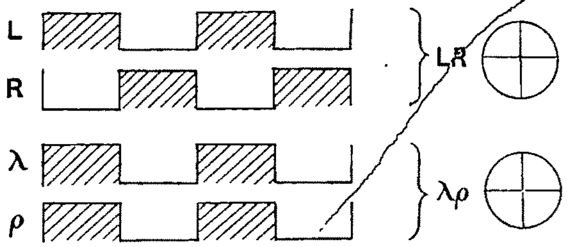

Diagram 2. Exp. 1.

The diagram makes the lower composite image the 'synchronous' one, but in the series of observations the 'synchronous' was sometimes the lower, sometimes the upper, and the observer was never informed which it might be. Sometimes the observations were made on the transition from flickering to unflickering sensation; more often they were made conversely on the transition from unflickering to flickering sensation: the observer in the latter way had a more neutral line of approach to the critical observation. For it was found that when compared under rates of intermittence giving marked coarse flicker in both images, all observers found the flicker "less" in the 'alternate' than in the 'synchronous' combination (*v. infra*, p. 57). This marked difference however lessened progressively as the frequency of intermittence increased toward fusion point. The difference at the lower centre was under foveal gaze would flicker perceptibly when the foveal gaze was fixed half-way down the interval between the two disc images. Exner[1] and Charpentier[2] have pointed out that the peripheral retina is more sensitive to flicker than the central, and Exner has shown that an area only 1330 μ from the fovea is more sensitive in this respect than the fovea itself. In the observations now in consideration the flicker sensitivity of points only 750 μ from the foveal centre was found perceptibly greater than that of the foveal centre itself. And this distance was found to reveal a perceptible difference, whatever might be the particular radial direction followed from the fovea.

Marked difference was met in the frequency of intermission required to extinguish flicker in the same physical light for different observers. The amount of individual difference is exemplified by the following es of intermission all observed on the same occasion upon the same

TABLE I.

Observer	No. of phases per sec. to extinguish flicker	Remarks
G. C.	124·3	Emmetropic
C. S. S.	117·8	Myopic 3 D.
B. M.	115·4	Emmetropic
J. S. M.	106·2	Myopic 5·5 D.
A.	94·5	Hypermetropic 2 D.

The abnormal refractions had been corrected by lenses. These differences remained characteristic between the observers A., J. S. M., C. S. S., and G. C., throughout two years during which the observations were in progress.

How regularly these individual differences are maintained can be judged from the following figures taken almost at random from my notebook, and obtained without any intention of instituting a comparison from two observers on occasions about a fortnight apart, examining the same electric light under similar conditions.

TABLE II.

G. C.	91·26	C. S. S.	88·40
,,	91·00	,,	87·88
,,	92·04	,,	86·84
,,	94·81	,,	86·94
,,	95·81	,,	91·00
,,	92·92	,,	87·88

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[1] *Sitzungsb. d. k. Akad. d. Wissensch. Wien*, 1868, Bd. LVIII. Abth. 2, S. 601.

[2] *Arch. d'ophthal. Paris*, 1890, Tome x. p. 340.

speeds inclined the observer to expect that complete extinction of the flicker would disappear the more readily in the image which at slow intermissions seemed to flicker the less. It was not easy to be sure that this bias was set aside in forming judgment on the final point of extinction of flicker in the two images. Usually, therefore, judgment was asked under conditions in which both images started perfectly free from flicker, the rate of intermittence being from the outset higher than necessary for extinction of flicker.

The judgment then given has been almost uniformly that there does exist a *very small* difference between the frequency of intermittence required for extinction of flicker in the ‘synchronous’ and ‘alternate’ combinations respectively. The difference is that in he ‘alternate’ combination flicker disappears at a slightly lower uency of intermission than in the ‘synchronous.’ All observers [illegible] that directly the frequency of intermission extinguishes flicker [illegible] the discs the appearance of both is indistinguishably similar, [illegible] there is then nothing to choose between the brightness of [illegible]o.

[illegible] s difference in frequency for flicker extinction is very small in amount. ome estimate of its amount, though it is too small to be measurable easily, [illegible] be gathered from the following observations. The observations have been de with various intensities of brightness of image, and various speeds of nslation of the contours separating the phases, hence the various absolute values, t the conditions for comparison between the two columns in the table have always een maintained.

TABLE III.

	No. of phases per sec. just extinguishing flicker	
Subject of observation	Synchronism of similar phases in r. and l. retinae	Exact alternation of phases in r. and l. retinae
S. C. M. S.	97·3	95·8
B. M.	122·3	118·9
G. C.	123·0	119·8
,,	93·3	90·6
,,	73·4	71·8
C. S. S.	72·1	69·8

For almost all persons whom I have examined, a spot intermittently illuminated at a frequency of intermission just sufficient to extinguish flicker in it, when looked at with one eye only, still flickers slightly when looked at with both eyes. A like phenomenon is noticed by most observers when examined by the arrangement (Experiment 2), represented by Diagram 3.

The binocular arrangement then is said by them to require a just slightly higher frequency for extinction of flicker than does the uniocular. Again, if under a frequency of intermission just securing extinction of flicker in either of the component uniocular images separately, one of these images, previously screened off, is readmitted,

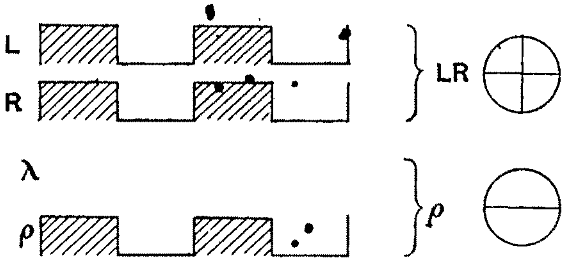

Diagram 3. Exp. 2.

so that the pair act together with a synchronous arrangement
a trace of flicker appears at once in the binocular image. It
urged that this is due to the fresh retinal area being more se
to flicker, and it is true that the flicker so introduced tends soo
become less, but a residuum of the phenomenon seems to remain.

Experiment 3. Conversely, under the arrangement indicated in Diagram
number of the persons examined, but not all, decide that the binocular image requ
for extinction of flicker a just slightly lower frequency of intermittent illuminati
than does the uniocular. Also, a number of these persons, though not all, find tha
when the 'alternate right and left' combination is observed under a frequency o
intermission of illumination just sufficient to extinguish its flicker, the screening
out of one of the component uniocular images brings with it a slight appearance of
flicker.

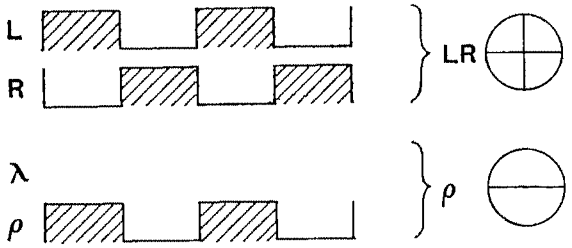

Diagram 4. Exp. 3.

From these observations it appears that similar phases of flickering illumination if timed to fall coincidently on conjugate retinal areas do *very slightly reinforce* each other in sensation, and if timed exactly alternately do very *slightly mutually* *reduce.* But the *broad outcome of the above experimental observations is that* so far from bright phases at one eye effacing dark phases at the corresponding spot of the other eye, *there is hardly the merest trace of any such interference.* To judge from its absence of influence on the flicker rate, the dark phase incident at retinal point A' does not, as regards sensual result, modify the bright phase synchronously incident at the conjugate retinal point A, and conversely. If the brightness of the bright phase or the darkness of the dark phase were lessened at A by A', the rate of frequency of stimulus requisite for extinction of its flicker must fall. But except in minute and perhaps equivocal degree it does not alter.

It may be that the alternate light and dark used in my experiments were of grades too intense to allow of their facile conjugate combination. Two greys not very dissimilar in tone and bounded by contours not disparate, fuse as we know to an intermediate grey (Fechner's paradox) without 'rivalry.' 'White' and 'black' on the other hand fuse with difficulty, are less congruent binocularly and tend to ccasion merely an oscillating form of sensation, 'retinal rivalry.' But in none of [illegible] observations near the point of extinction of flicker has evidence of retinal rivalry een the already fairly-similarly luminous fields been noticeable.

[illegible] incidentally repeated these experiments under conditions of ion and retinal adaptation to light differing from those employed tically for the rest of the inquiry. With quite low luminosities [illegible] e disc-images and under dark-adaptation of the eyes—after half-our or longer in darkness—the results obtained have conformed with se found without dark-adaptation and under the much brighter illu mations otherwise systematically used. The LR and $\lambda\rho$ images have own no distinct difference between them as to frequency of inter-ission required to extinguish flicker in each respectively. Several imes the 'synchronous' combination has appeared to have its flicker extinguished slightly the less readily, just as noticed in the observations with higher luminosities and light-adapted eyes: but the difference when perceived has been so slight as to be perceptible with difficulty. The frequency of intermission required for flicker-extinction has under the low luminosities been of course extremely slow. A cylinder with only three openings per revolution was used in order to give more quickly moving edges to the alternate lights and shadows. There was also under low luminosities and dark-adaptation no distinct difference of flicker-extinction frequencies noted between the two discs when observed not on the central retina but outside that region, *e.g.* with either R or L image between the macula and the blind-spot, and somewhat nearer the latter. But I have not systematically pursued observations with various peripheral regions of the retina.

As far as sensual effect goes, the light phases at the one eye practically do not, therefore, interfere or combine at all with the coincident dark phases at the other; and conversely. Nor do they, in the alternate left and right arrangement, add themselves as a series of additional stimuli to the like series of stimuli applied at the other eye. If they did the revolution rate of the cylindrical shutter required for extinction of flicker in the upper binocular image *LR*, Diagram 2, would fall far below that required for extinction in the uniocular. This it does not do. It does not even fall at all, apart from the minute difference noted by some persons as mentioned above. A similar result is obtained under the, in some ways more decisive, conditions (Experiment 4), represented in Diagram 5.

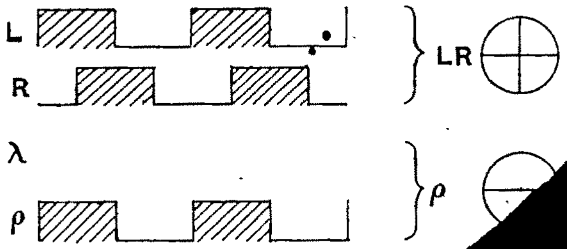

Diagram 5. Exp. 4.

With this arrangement no observer in
with certainty detected any difference a
and binocular images in regard to ei
flicker when moderately coarse, or t
for flicker-extinction. This arrang
crucial for deciding the point.
arrangement (Diagram 2, L. R.,
change of phase falling together i
not matter as regards flicker-sen
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synchronous, it might t
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arrangement. The physiological stimulation it delivers to the conjugate retina is by any mode of count delivered at twice the rate of delivery for either retina considered apart from its fellow. *Yet the rate of revolution of the cylindrical lantern required to extinguish flicker in this experiment remains for the binocular image the same as for the uniocular.*

There arises the question whether we may regard the dark field covering the area correspondent with that to which in the other retina a bright image is presented, as non-existent visually. That assumption has been made above, and is indicated in the diagrams (Diagrams 3, 4, 5). In them, where one image is represented as uniocular, the conjugate area of the other retina is left out of the diagram altogether, as though the latter retina were non-existent, or [illegible] the time being blind. This seems permissible, because in all the [illegible]iments in which a binocular image was compared with one [illegible] to be purely uniocular, great care was exercised to ensure [illegible] all trace of detail or contour from the homogeneous darkness [illegible] the time over the whole of the other retina, except where lay [illegible] component of the compared binocular image. When all detail [illegible]ntour were absent from the field containing this correspondent [illegible] when in fact that field was perfectly void of contours, and homo[illegible]ous, unchanging and borderless, it was found that it mattered [illegible]e what *depth* of darkness it might have; it might be a shade of [illegible]ey or even a fair white, without perceptibly influencing the sensual [illegible]ibrations given by the flickering image before the other eye. The absolute blankness of the field seemed to unhitch the region of retina which it covered from higher cerebral connexions, at least to prevent its reactions from contributing to consciousness. The condition seemed comparable with the familiar disability to see the dark field presented to one closed eye, when with the other eye the observer regards a detailed image[1]. As will be shown in the next section an unflickering image presented to one eye damps the flickering of an intermittent image concurrently presented to the corresponding area of the other. For these reasons the visual image resulting from the presentation of the bright disc to one eye only, as in the arrangements shown by Diagrams 3, 4, and 5, was regarded as being a truly uniocular product, uncomplicated by any component from the other retina. The corresponding area of this latter was considered as for the time being out of action as regards sense, not so much by darkness as by ensuring borderless void homogeneity of field,—as when eye-closure affords visual rest. Under this blankness the 'retinal-points' become unhitched from the running machinery of consciousness, if—and this is essential—the 'corresponding' retinal area be concurrently under stimulation by a defined image. McDougall's[1] principle of competition for energy between associate neurones seems at work here, for with *both* eyes shut the dark blankness of eye-closure does become *visible*. Even with one eye open, if its field be undetailed and homogeneous, glimpses of the 'Eigenschwarz' of a closed eye become obtainable (Purkinje, Volkmann, E. Hering).

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[1] Cf. Helmholtz, *Physiologische Optik*, 2 Auf. S. 916.

The stimulations of the two retinae being thus accurately conversely timed, some interference of the flicker sensations so generated might be expected to be discoverable. But the above experimental evidenc [illegible] indicates absence (practically entire) of any interference between t [illegible] flicker processes so initiated. The right and left 'corresponding r [illegible] cerebral points' do not when tested by flicker reactions be [illegible] though combined or conjugate to a single mechanism. Th [illegible] reactions retain individuality as regards time relations [illegible] completely confluent as judged by reference to visual space.

SECTION II. **Asymmetrical Flicker.**

In the foregoing experiments the sensual flicker reactions engend [illegible] at 'corresponding' areas of the two retinae appear (almost entire [illegible] without influence one upon another. But in the experiments no [illegible] following the flicker test reveals very considerable mutual influence between reactions initiated at the corresponding areas.

Suppose (Experiment 5) two binocular images LR and $\lambda\rho$ similarly combined from similar uniocular components, all individually equal in brightness and in intermission frequency. Suppose that of the components of one pair ($\lambda\rho$) one (ρ) be replaced by an intermittent uniocular image (ρ'), of the same physical brightness as that giving the visual image ρ, but of considerably higher intermission frequency. In ρ' all flicker will disappear at slower speeds of revolution of the lantern than those required to extinguish flicker in L or R or λ. Diagram 6 represents the arrangement.

The frequency of intermission required to extinguish flicker in $\lambda\rho'$ is then found to be much lower than the frequency required for extinction of flicker in *LR*, or in *L* or *R* or λ separately. Thus the frequency for extinction of flicker in $\lambda\rho'$ was found (observer H. H.) to average 52·2 phases per sec., as against 61·9 phases per sec. for *LR*, or for *L*, *R*, or λ separately. With another observer (S. C. M. S.) its extinction occurred on the average at 48·3 phases per sec. as against 58·5 per sec.

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[1] *Mind*, 1902, N.S. XI. p. 316.

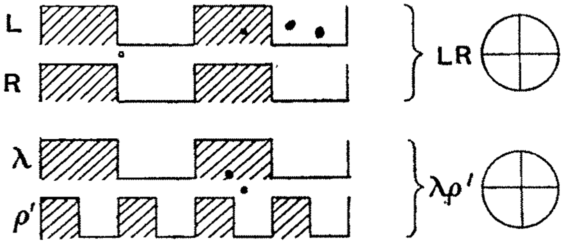

Diagram 6. Exp. 5.

[illegible]g image ρ' out of the binocular combination $\lambda\rho'$, when the [illegible]f intermission was just high enough to free the $\lambda\rho'$ image [illegible]ker, at once brought flicker into it; this disappeared immediately [illegible] ρ' was readmitted to the combination.

[illegible]n this instance the intensity chosen for the steady illumination [illegible]the conjugate area was equal to that employed for the uniocular [illegible]ickering image. The durations of the light phases and the dark per revolution of the lantern were equal, and the light and dark phases of the same intensity in both. But the phenomenon obtains also when the steady uniocular image is less bright (Experiment 6) or more bright (Experiment 7) than the flickering uniocular with which it is combined. The following are examples illustrating this.

Experiment 6. In the balanced pair of binocular images *LR* and $\lambda\rho$ made of carefully equalised intermittent uniocular images *L*, *R*, λ and ρ, the uniocular image ρ was replaced by one, ρ', of five times greater rapidity of intermission and giving a steady image of only $\frac{1}{5}$ the brightness of the images *L*, *R*, λ and ρ when steady. The frequency of intermission required to extinguish flicker in the binocular image $\lambda\rho'$ (Diagram 7) was then found to be 72·1 phases per sec., whereas in *L*, *R*, and in λ, *L*, and *R*, separately it was 75·5 phases per sec. as it had been in the previous $\lambda\rho$. The steady sensation from image ρ' therefore damped the vibration of the flickering sensation from the conjugate spot under image λ by an amount represented by 3·4 phases per sec. This was for the observer G. C. For another observer, S. C. M. S., the difference was greater, namely, six phases per sec., the flicker extinction rate being 56·4 phases per sec. for image $\lambda\rho'$, and 62·4 phases per sec. for images λ, *L*, *R*, or *LR*, or the previous $\lambda\rho$.

Again, for the same observer S. C. M. S. when the brightness of image ρ′ was further reduced to $\frac{1}{12}$ that of the λ and the other uniocular images, the flicker extinction rate for λρ′ became 54·3 phases per sec. as compared with 61·2 phases per sec. for *LR* or the uniocular images λ, *L*, *R*, taken separately. In all these observations the image λρ′ was visually distinctly less bright than *LR*, or λ, or *L*, or *R* taken singly (cf. Fechner's paradox).

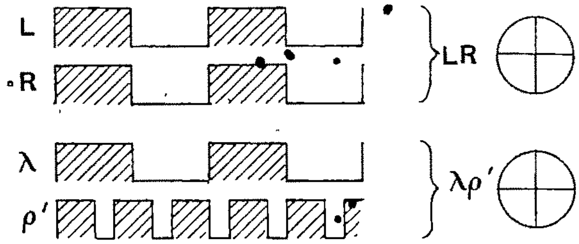

Diagram 7. Exp. 6.

Experiment 7 illustrates an observation in which for the ima[illegible] binocular combination λρ an image ρ′ was substituted of thre[illegible] frequency of intermission and giving a steady image of one-fifth grea[illegible] than the image *L*, *R*, and λ when steady. It was then found that the fr[illegible] intermission required to extinguish flicker in the binocular image λρ′ (Di[illegible] was 57·8 alternate equal phases (of λ) per sec. Whereas in *LR*, and in λ, *L*, [illegible] taken separately, the number of such phases required was 63·6 per sec.

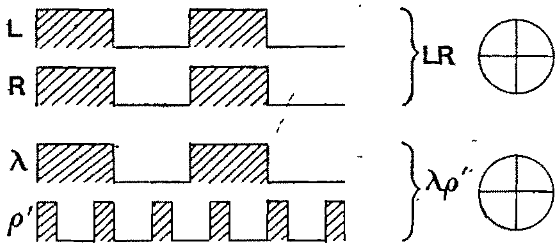

Diagram 8. Exp. 7.

The image λρ′ was distinctly brighter visually than was *LR*, or any of the uniocular images λ, *L*, and *R*.

Again (Experiment 7 A) with image ρ′ steady and of twice the luminosity of λ, or *L*, or *R*, the binocular visual image λρ′ loses flicker at an intermission of 80·3 phases per sec., but binocular image *LR* not until 85·9 per sec.

These observations show, as did the observations represented by Diagram 6, that it is not merely the reduction of brightness in the combined image $\lambda\rho$ in the arrangement shown by Diagram 7 that lessens the flicker in the latter. In fact in the observations on the plan illustrated by Diagram 8 we have the, for flicker photometry, interesting case of a brighter intermittently illuminated surface flickering less than a duller one.

Here the conditions of experiment at once suggest a possible explanation. It is as though the dark phases of the intermittent illumination of the left retina (*e.g.* in Exp. 7) were lightened by the contemporary illumination of the 'corresponding' area of the other retina. Before accepting this plausible supposition it is however necessary to consider two objections. The illumination at the 'corresponding' spot obtains not only during the dark phases of the intermittent images at the other retina, but during the light phases lso. If it brightened these as much as it lightened the dark phases [illegible] intensity of intermittence would remain practically unaltered, and [illegible] ove supposition could not explain the reduction of flicker which [illegible] illumination of the conjugate area causes. But experiment [illegible] he brightness of a binocular image in the central field of [illegible] apted eye is not the sum of the two component uniocular [illegible] When the two components are of equal brightness the brightness [illegible] the compound is hardly greater, sometimes not at all (*v. infra*, [illegible] n III.) perceptibly greater, than that of either individual con[illegible]ent.

[illegible] Indeed the resultant binocular brightness of two component uniocular rightnesses, of such order of intensity as used in these experiments (and with the eye not adapted for dark[1]), seems to lie near the arithmetic mean of the two components (*v. infra*, Section III. p. 50). The addition of the steady brightness at one eye to the dark phase of the intermittent at the 'corresponding spot' would according to that lighten the latter, and its addition to the bright phase would if of equal brightness with that leave it practically unaltered.

If to an intermittent uniocular image R a steady uniocular image L is added by binocular combination, under the rule just mentioned, it is obvious it will not matter what, within wide limits, is the relative brightness of L to R; L's power to diminish the flicker of R will for all L's values remain about the same.

So far as flickering of R depends upon difference in brightness between its successive light and dark phases, this difference will be lessened practically to a similar extent, whether the steady brightness of *L* be less than, equal to, or greater than *R*'s brightness. A paper disc, made up so as to represent in its concentric circles the hypothetical *LR*'s of cases iii. and iv. of Diagram 9, gives by diffuse daylight no clear difference of flickering of the two. But I have not yet examined the case *LR* iii. of Diagram 9 in the rotating lantern itself. I have however determined experimentally the amount of decrease of flicker in a uniocular image *R* under binocular combination with an unflickering

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[1] Piper, *Zeits. f. Psych. u. Physiol. d. Sinnesorgan.*, XXXII. 161, 1903.

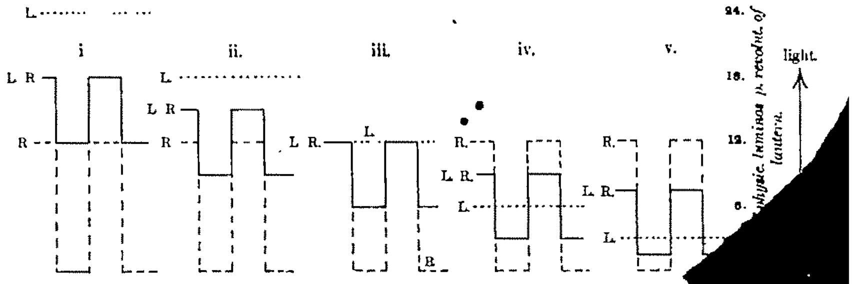

Diagram 9. The continuous line indicates the changes in the binocul broken line those in the *R* eye image, the dotted line the steady lumin *L* eye image.

image *L*, of physical luminosity respectively $\frac{1}{4}$, $\frac{1}{2}$, equal to, $1\frac{1}{2}$, twice, that of *R*. The method followed was to take a 'standa binocular intermittent image $\lambda\rho$, made up of two equal intermitte left and right uniocular components λ and ρ. Close beside $\lambda\rho$ in the binocular field was set an image *LR*, made up of a uniocular component *R*, exactly like each of the components of $\lambda\rho$, and of a component *L* of perfectly unflickering brightness. The physical luminosity of λ and ρ and *R* being taken as value 6, that of *L* was in the five cases 12, 9, 6, 3 and 1·5 respectively.

TABLE IV.

	Physical luminosity		No. of observations (G. C. and C. S. S.)	Mean reduction observed in fusion point for flicker of R, expressed in phases per sec.
	Image L. (steady)	Image R.		
i.	12	6	12	7·55
ii.	9	6	18	7·42
iii.	6	6	24	8·54
iv.	3	6	24	6·25
v.	1·5	6	28	8·20

These grades were except the first obtained by using an episkotister in front of the one of the lantern windows chosen to furnish image L, the physical luminosity numerically denoted above therefore involves a time unit, *e.g.*, a second, or a revolution, either of the cylindrical lantern for ρ, λ or R, or of the episkotister for L. Lest it be thought that the physical intermittence of the unflickering image was possibly of influence or a possible complication, it must be stated that the rate of the episkotister was far above that necessary to extinguish all trace of flicker from the visual image. Also, it was by experiment found that increasing or diminishing the speed of the episkotister by half did not modify the result, so long always as its speed remained above that necessary to extinguish flicker. Moreover for the determination where $L=12$ no episkotister but a steady light was used, and yet the measurement obtained was the same.

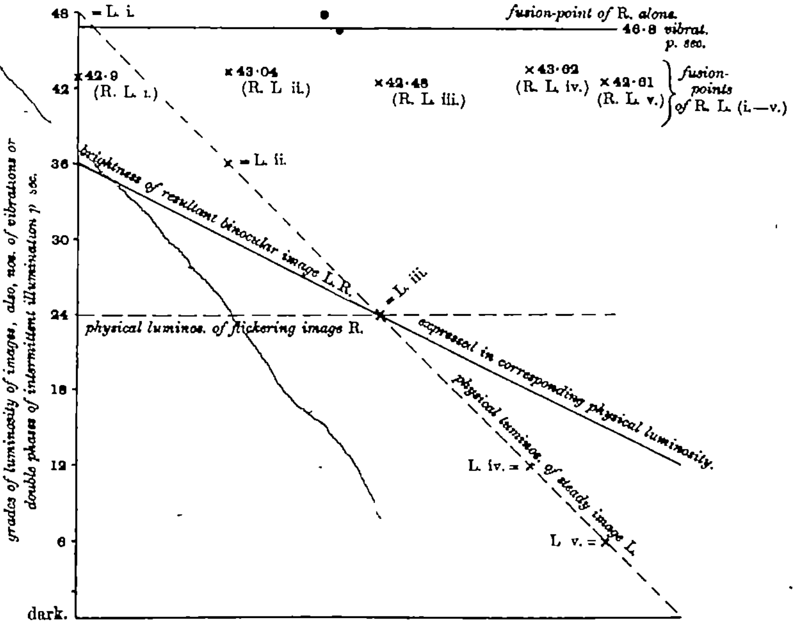

Diagram 10. The heights of the Ls represent their physical luminosities not the frequencies of intermittence for extinction of their flicker.

The amount by which the fusion-point (*i.e.* the point of speed at which flicker was extinguished) for image LR lay lower than that for the standard $\lambda\rho$, or for R, was taken as measure of the influence of the continuous steady image upon the flickering one. The results observed are given in Table IV. opposite.

The difference between the extremes of these measurements lies within the range of error of the observations. I conclude, therefore, that *the reduction of the flicker of an intermittent uniocular image by binocular combination of it with an unflickering uniocular image remains practically the same over a considerable range of variation of luminosity of the unflickering image.*

To return therefore to the plausible suggestion above, that in these experiments we have evidence of mutual interference between the purely physiological processes initiated at the corresponding spots of the right and left retinae; the suggestion meets no direct contradiction, but rather appearance of support from the determinations.

But, on the other hand, the above result at once suggests that the binocular product from a uniocular flickering and a uniocular unflickering image arises by a synthetic process cognate to that which produces from a pair of individual uniocular brightnesses a binocular brightness near the arithmetic mean of the brightnesses of the two components. The rule of combination exemplified by these latter (*v. infra*, Section III.) finds little solution by appeal to summation or interference of retinal and purely physiological processes. It seems rather a psychical synthesis that works with components that have already attained discrete sensual existence, *i.e.* with processes open to introspection as psychical entities. It is as though visual 'flicker,' or conversely 'steadiness,' once obtained, no matter physiologically how, from one retina, has then, when seen, to compete perceptually with comparable qualities of any visual image referable to the corresponding area of the other retina.

Moreover, the supposition that the sensual reaction caused by a steady image acting at one of the pair of 'corresponding' areas, is interfering with or combining with the individual phases of reaction to the intermittent image at the fellow area, is exactly the supposition that the observations dealt with in Section I. indicate to be untenable.

SECTION III. **Uniocular and Binocular Comparisons.**

With intermittent lights throughout a wide range of ordinary intensities Talbot's law is unimpeachable for the single eye; and also for the two eyes if employed together under, as is usual, arrangements practically equivalent to the 'simultaneous' right-left method of Section I. It is interesting to discover how far the double retina will still observe Talbot's law when subjected to treatment that, if the retina did then observe the law, would readily reveal its integration to a functionally single retina. In other words, under a rapidly repeated stimulus, when one incidence of that stimulus has acted on a retinal point the question is: how far is it the same thing for visual brightness, whether the next incidence be upon the same retinal point or upon the twin point in the other retina? How far can the double retina, when functioning for singleness of perception in binocular vision, be considered as functionally combined to a single retina, and how far does it then react as does a single retina, if examined for Talbot's law?

The 'alternate left-right arrangement' of Section I. (Experiment 1, *LR*) supplies the required method of stimulation. With speeds of revolution of the lantern too high to allow flickering, the binocular image *LR* (Diagram 2) is seen to appear of equal brightness with $\lambda\rho$, and with the uniocular images λ or ρ taken singly. Therefore, in the above sense, Talbot's law not only does *not* hold for the double retina considered as functionally single, but it yields even no trace of observance of the law. The two corresponding points are therefore in this respect not integrated to a single retinal surface.

It was often noted that with all four lantern images of equal luminosity, using intermission-frequencies too rapid to allow flicker, the brightness of the binocular combination of any two did not distinctly exceed that of the uniocular. In certain instances the binocular combination did appear just distinctly the brighter. This was for instance the case when of the four lantern images the two on the same horizontal level were combined by simple convergence. This excess of brightness is the well-known phenomenon examined by Jurin[1], Harris[2], Fechner[3], Aubert[4], Valerius[5], and others. But there occurred frequent instances in which no excess was observed in the brightness of binocular combinations over that of their carefully balanced uniocular components. In the observations of the present inquiry the brightness of the physical images was always much above the threshold of the light-adapted eye; and no systematic observations were made with the eye dark-adapted. To obtain good conditions for comparison of the brightness of the binocular and uniocular images observed the following arrangement was employed.

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[1] Smith-Kästner, *Lehrbegriff der Optik*, 1755.

[2] *Opticks*, 1775.

[3] *Abhandlung d. Akad. Wiss., Leipzig*, VII. 428, 1860.

[4] *Physiologie d. Netzhaut*, S. 287, Breslau, 1865.

[5] *Poggendorff's Annal.* Bd. 150, S. 17, 1873.

Experiment 8. Two images *LR* and $\lambda\rho\frac{1}{2}$ were placed in the visual field for mutual comparison. *LR* was composed of left-eye and right-eye equal and corresponding disc-shaped images as in previous experiments. $\lambda\rho\frac{1}{2}$ was composed of a left-eye image similar to *L* and *R* except that it lay just above or below them in the visual field. With λ's right half was combined the image of the right half of a lantern image similar again to the others, except that its left half was screened absolutely off into the blank undetailed darkness of the general field. When this was done the two opposite visual images *LR* and $\lambda\rho\frac{1}{2}$ regarded under perfectly steady ocular fixation, were stable, and no difference of brightness was discernible between them. Moreover no join was seen between the halves of $\lambda\rho\frac{1}{2}$ and no difference of brightness between the halves. After prolonged inspection of them rivalry became troublesome; but a judgment could be clearly arrived at before that happened.

In this experiment it might possibly be that equality of brightness between the halves of $\lambda\rho\frac{1}{2}$ was due to image $\rho\frac{1}{2}$ not really being in consciousness at all during the comparison. The image might possibly lapse under competition with the partly dissimilar correspondingly placed left-eye image λ. Some of the experiments carried out by McDougall[1] give validity to such possible objection. The perceptibility of the horizontal bar in the right half of the image $\lambda\rho\frac{1}{2}$ was guarantee however that at least part of the uniocular image $\rho\frac{1}{2}$ was present. But to ascertain more surely whether image $\rho\frac{1}{2}$ was really during the visual equation co-operating in consciousness with λ the following further arrangement was employed.

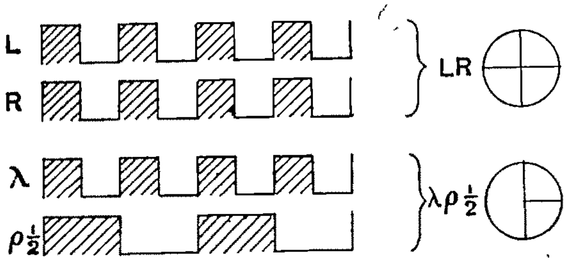

Diagram 11. Exp. 9.

Experiment 9, (Diagram 11). With the revolving lantern so arranged that images *L*, *R*, λ and $\rho\frac{1}{2}$ were all of equal brightness when steady and unflickering, $\rho\frac{1}{2}$ was given a lesser frequency of intermission, so as to flicker while the others did not. A speed of revolution of lantern was then used at which just a trace of flicker was perceptible in $\rho\frac{1}{2}$ when binocularly combined with λ. The equation $LR=\lambda\rho\frac{1}{2}$ was then found to hold while flicker was still just traceable in the right half of $\lambda\rho\frac{1}{2}$. There was then no join seen between the halves of $\lambda\rho\frac{1}{2}$ nor any difference between the brightness of the halves. So long as ocular fixation was steady no rivalry disturbed the observation.

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[1] *Mind*, 1901, N.S. x. p. 68.

In this case there could, I venture to think, be no question but that the one half of $\lambda\rho\frac{1}{2}$ was truly binocular, for the trace of flicker was perceptible during the actual performance of the comparison. Yet no difference of brightness was perceived between *LR* and $\lambda\rho\frac{1}{2}$, and the two lateral halves of $\lambda\rho\frac{1}{2}$ compared together were of like brightness.

Even where the binocular image has shown the well-known slight excess of brightness over its uniocular components it, under some conditions (*v. supra* 'alternate' arrangement), flickers no more or even less than they.

It is doubtful therefore to me whether the slight excess in brightness of the binocular image over its two equal uniocular components is really explicable as summation of the intensities of the reactions at the corresponding spots of the two retinae. Valerius[1] measured the increase to be one-fifteenth of the brightness of the uniocular image. Aubert's[2] diagram gives it as less than one-thirtieth. Aubert says it is not perceptible with brightnesses greater than that of white paper in diffuse daylight indoors[3].

In certain modes of experiment a uniocular image used as standard for comparison might itself be suffering some reduction in brightness owing to slight combination with the dark field presented concurrently at the corresponding retinal area. But 'rivalry' should reveal such influence. A better definition and greater vividness of detail assured by better accommodation and convergence under binocular regard, might possibly give an appearance of greater brilliance and intensity. But these are only suggestions.

I conclude that, with the intensities of illumination used in this research, although a binocular image does sometimes appear of slightly greater visual brightness than either of two similar uniocular images composing it, more often it has a visual brightness not perceptibly different from that of either of its two co-equal uniocular components. The case then falls within a general rule regarding binocular brightness attested by all observations that have borne on that subject throughout the present inquiry. Disc images of homogeneous surface, except for a cross-line, have been the objects of comparison. The rule was in my preliminary paper[4] stated thus: "the physiological sum of two luminosities, perceived through conjugate retinal areas, is of a value intermediate between the individual values of the two component luminosities." I think it better stated as follows: *a binocular brightness compared with its uniocular components is of value not greater than the greater of those, nor less than the lesser of them; when free from oscillations of rivalry its value is somewhat, but not far, above the arithmetic mean of the values of the two uniocular components as expressed by the measures of the physical stimuli yielding them.*

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[1] *Poggendorff's Annalen*, Bd. CL. S. 117, 1873.

[2] *Physiologie d. Netzhaut*, S. 286, Breslau, 1865.

[3] *Physiologische Optik*, S. 500, Leipzig, 1876.

[4] *Proc. Roy. Soc.* LXXI. p. 75, July, 1902.

The various combinations cited in Experiments 2, 3, 4, 5, 6, 7, and 8 have all, when steady and unflickering, given brightnesses illustrating the above rule. Other illustrations are

$\lambda = 1000, \quad \rho = 250, \quad \lambda\rho = 680,$
$\lambda = 1000, \quad \rho = 350, \quad \lambda\rho = 750,$
$\lambda = 1000, \quad \rho = 550, \quad \lambda\rho = 835,$
$\lambda = 1000, \quad \rho = 750, \quad \lambda\rho = 920,$
$\lambda = 1000, \quad \rho = 1000, \quad \lambda\rho = 1000.$

But I have not worked with combinations where the physical luminosity of one uniocular component has been less than $\frac{1}{15}$th the physical luminosity of the other. It was near these that Aubert, and just beyond these that Fechner, noted decline of the darkening effect of the darker component. In my own few observations beyond that point the oscillations of rivalry have made judgment difficult. The more manageable examples are but demonstrations of 'Fechner's paradox,' and fall under the above general rule. Hering[1] suggests that rivalry is really occurring even with similar right and left uniocular images; he says these react according to a law of 'complemental shares,' and offers a theory, such as the name he gives implies, in explanation of the phenomenon. My Experiment 9 seems to offer difficulty to such a view.

In the light of the above formulated rule, the difference in visual brightness between binocular combination of two images and their effect when superposed on a single retina, is not surprising, although great. A single instance will suffice as illustration here.

Experiment 10 (Diagram 12). *L* and *R* are co-equal uniocular images, each of physical luminosity higher as 33 to 25 (roughly represented in the Diagram) than that of either of the two images λ and ρ, *co-equal uniocular components of the* binocular image $\lambda\rho$. L seen beside λ is visually much the brighter; so also when seen beside $\lambda\rho$.

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[1] *Beiträge z. Physiologie, Heft v. S. 310, Leipzig, 1864.*

These images were reflections of the lantern images, thrown by first surface mirrors on a Lummer-Brodhun mat white porcelain screen. The mirrors were fitted to 'Basler Stativs,' and the fine adjustments of these allowed the images L and R (or λ and ρ), to be (1) placed side by side or (2) physically superposed, on the screen. The physically combined image $\lambda+\rho$ seen uniocularly was much brighter than $\lambda\rho$ binocular combination. $\lambda+\rho$ physically combined, was also seen much brighter than LR, the binocular combination of L and R.

Again, binocular combination of a less bright image with a more bright gives a visual image of less brightness than the latter (as stated in the rule above). But the application of the less bright to the same uniocular area as the more bright gives a visual image of greater brightness still.

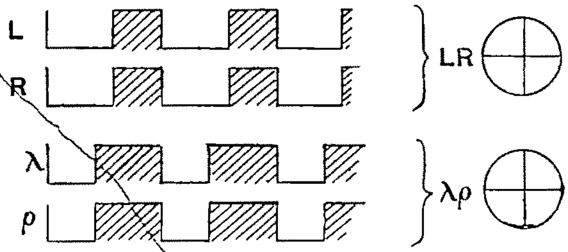

Diagram 12. Exp. 10.

As above described, a steady image presented on an area of one retina 'damps' the flicker of a flickering image concurrently presented at the corresponding area of the other. A steady image actually physically superposed on the same retinal area as a flickering one also reduces the latter's flicker: this is of course in accordance with Weber's law. The modes of interference seem however incomparably different in the two cases; and experiment shows that the two interferences are often of quite different value.

Experiment 11. A pair of the lantern images reflected as in the last cited experiment. The images can be placed on the screen (1) side by side or (2) superposed. The rotating lantern is so adjusted that one image, L, has twice the physical luminosity of the other R, when flicker has been extinguished in the latter. L is steady and without flicker. Image R viewed by right eye loses flicker at an intermission frequency of 68·6 phases per sec. Binocular fusion of R viewed by right eye with L viewed by left eye produces a visual image whose flicker is lost at 65·9 phases per sec. Physical images L and R superposed on screen and viewed by right eye give a visual image whose flicker is lost at 59·8 phases per sec.

Experiment 11 A. Observer G. C.

Binocular fusion of *R* and *L* gives flicker extinction at 65·5 phases per sec.
Physical " " " " " " " " 59·4 " "

Observer R. S. W.

Binocular fusion of *R* and *L* gives flicker extinction at 55·9 phases per sec.
Physical " " " " " " " " 54·7 " "

Experiment 11 B. *L* image has ½ the luminosity of *R* when *R* is also unflickering:

Observer R. S. W.

Binocular fusion of *R* and *L* gives flicker extinction at 106·6 phases per sec.
Physical " " " " " " " " 100·3 " "
R separately gives flicker extinction at.................... 113·3 " "

Observer S. C. M. S.

Binocular fusion of *R* and *L* gives flicker extinction at 91·0 phases per sec.
Physical " " " " " " " " 87·8 " "
R separately gives flicker extinction at.................... 99·3 " "

Finally, to touch on the subject of 'predominance of contours,' the facts, established by so many workers, are among the most significant concerning the difference between binocular and uniocular fusion of visual reactions. I will merely give here an illustration which seems specially instructive for the point under inquiry.

Experiment 12. A steady unflickering disc-shaped image *L* is present to the left eye: across the disc is a narrow dark line. An image *R* of similar size and shape but without the dark line is presented to the corresponding area of the right eye. If the luminosity of *L* is progressively diminished, a value of luminosity is reached at which its cross line, though visible when *L* alone is observed (*e.g.* right eye closed) is lost or uncertain in the binocular image *RL*. This reduction of the luminosity of *L* much exceeds the reduction at which its cross line is lost when image *R* is concurrently thrown on the same area of the same retina, *i.e.* left retina. Thus, in one experiment the diminution of luminosity of *L* required for loss of the cross-line under physical superposition of *R* and *L* on the same retina was 84 per cent., while the diminution of luminosity of *L* required for loss (or great uncertainty) of the line in the binocular image was 96 per cent.

Not only the ease, but the *mode* of disappearance of the cross-line, was significantly different in the two cases. In the "physical superposition" the dark line became gradually thinner and fainter, and finally imperceptible, as the image *L* was lessened in luminosity. In the case of binocular fusion the dark line oscillated out of and back into perception more and more, the disappearances predominating more and more, as the darkening of *L* proceeded. At a reduction of 84 °/$_o$ of the luminosity of *L* the cross-line was steady, dark, and sharp in the binocular image.

SECTION IV. **General Conclusions.**

The aim of this inquiry was information as to the nature of the conjunction between the uniocular components in certain simple binocular perceptions. The question concerns the nature of the tie between 'corresponding retinal points,' meaning by 'retinal point,' as said at the outset, the retino-cerebral apparatus engaged in elaborating a sensation in response to excitation of a unit area of retinal surface.

That a perception initiated from corresponding retinal points is commonly referred without ambiguity to a single *locus* in visual space has often been regarded (Newton[1], Wollaston[2], Rohault[3], Joh. Müller[4]) as evidence of community of the nerve apparatus belonging to the paired retinal points. Their visual image appears *single*. Wollaston supposed the twin points attached to one and the same nerve-fibre, which bifurcated at the chiasma. Rohault and Müller supposed the points to be served by twin fibres "from one and the same ganglion-cell in the cerebral substance." Later (cf. Aubert), the visual singleness, spatial fusion of right and left impressions to a single perception, was taken to mean confluence of the nerve-processes, started in right and left retinae respectively, to "a single common centre or point of the sensorium[5]." The discovery later still that the fibre-tracts from corresponding halves of the retinae both go to the occipital region of one and the same hemisphere, has also been inferred to mean a spatially conjoint visual sensorium common to both retinae. But in such questions the inferences obtainable from comparatively rough anatomical features are crudely equivocal and often remote in bearing. Were there to exist such a common mechanism situate as a unit at conjunction of the two convergent systems and were phases of excitement timed so to arrive from one retina as exactly to fill pauses between excitations transmitted from the other, then there should be evidence of this in the time relations of the phenomena induced. The state of excitement should tend to be maintained across periods that would otherwise chequer it as pauses.

The retino-cerebral apparatus may be regarded as a structure of linked branching nerve-elements forming a system which expands as traced centrally from the retinal surface. It may be figured as a tree, with its stem at the retina and an arborisation spreading into the brain, its ramifications there penetrating a vast cerebral field, interlacing with others in a cerebral forest composed of nervous arborisations. The simile fails, because in the nervous forest the arborisations make functional union one with another. Is the intimate connexion between the perceptions adjunct to paired 'corresponding points' the outcome of a close concrescence of their neuronic or neuro-fibrillar arborisations, making of them practically a single upgrowth common to twin (right and left) stems rooted in the corresponding retinal units? If so, how low down, how close to their origin, are the twin systems grafted together, giving structural community to all the superstructure? Or suppose each 'retinal point' represented as a system of branched tubing, ramifying more and more as it passes inwards from the retina; do the reactions of paired 'retinal points' indicate that the two systems combine to a common one? If so, how early do their functional processes conjoin; how near to the retinal origin does the intercommunication begin?

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[1] *Opticks*, Quer. 112.

[2] *Philosoph. Trans. London*, 1824.

[3] *Physique*, I. 81.

[4] *Elements of Physiol.* Vol. II. p. 1199, Baly's edit. 1843.

[5] *Physiol. d. Netzhaut*, 1865.

Effects of excitation, represented as changes of pressure in the systems, valves preventing reflux toward their retinal ends, will, when similar stimuli are applied synchronously to the two stem-pipes, if their systems intercommunicate, reinforce each the other. Conversely, alternate stimuli will each tend to neutralise the effect of the other. There will be 'interference,' algebraic summation, compounding of vibrations, as with a medium subjected at once to two sources of vibration.

In the chain of nerve-elements attached to a sense-organ we infer in general that to the activities of the most peripheral links *per se*, psychical events are not adjunct. Psychical processes, beginning with least complex and ascending toward development through many grades, attach to the chain in such a way that for the simplest only the more peripheral portions of the chain need be connected with the sense-organ, while for the more and more complex the central portions in addition become more and more extensively involved. But in the higher reactions of definite psychical aspect, *e.g.* sense perceptions, the lower apsychical and less definitely psychical activities also are implicate. Where from two sense-organs, *e.g.* two units of retinal surface, the two nerve-chain arborisations are mutually connected, so that the lower activities of the one are communicated by low-level side-connexions to the elements forming the other, there analysis must fail to distinguish in the full reaction what higher components may be separately referable to one only of the two individual chains. The processes apsychical, or so indefinitely psychical as to baffle introspection, at root of the psychical processes amenable to introspection, must by their coalescence defeat attempt to trace the final psychical product to either of its two possible sources, so long as both sources are open for its origination.

Were the nervous reaction, initiated at the retina, early in its path along the retino-cerebral nerve-chain, to enter mechanisms common to both 'corresponding retinal points,' there must, under 'alternate' or 'synchronous' right-left arrangement of stimuli (Section I., Diagram 2), be a coalescence of events which, though apsychical in themselves, would involve subsequent confusion together of the sense-reactions of the two eyes. A state of things wholly different from this is revealed in the results above experimentally obtained. Talbot's law might in that case be expected to hold good for the paired corresponding points, functioning together, just as it does for the point of a single retina. That is to say, it might be expected to amount to the same thing, or approximately the same thing, whether two quickly successive flashes of a light fell both on one and the same member of a pair of 'corresponding points,' or whether the first fell upon one member, the second upon the other. But the experiments show that the effect in the two cases is widely different. In other words, Talbot's law is not applicable to the double retina, that is, to the two retinae functioning together in binocular vision. The experimental results go to disprove the existence of any such fusion or interference between the apsychical or even the subperceptual events arising from corresponding retinal points. At most they indicate hardly discernible traces of such interference (Experiment 1). Moreover they indicate on the contrary that such simpler forms of binocular perception as have been dealt with here are themselves fusions of elaborated uniocular *sensations*. Since left and right end results emerge pure, 'hybridisation' has not mixed the early stages in their evolution.

But the difference between the modes of stimulation left and right (Experiments, Section I.) is a difference that, although it should be potent if the left and right physiological machinery were conjoined to unity, should constitute no difference when left stimulation is compared with right stimulation by the perceptual product which each yields. *The left eye and right eye flickering visual images, each viewed singly, do* NOT (apart from the faint cross-line for recognition) *differ to introspection.* If the sensations derived from the left eye and right eye respectively appear under introspection indistinguishably alike, what ground is of conscious vision altogether. The inference is that only *after the sensations initiated from right and left 'corresponding points' have been elaborated, and have reached a dignity and definiteness well amenable to introspection, does interference between the reactions of the two (left and right) eye-systems occur. The binocular sensation attained seems combined from right and left uniocular sensations elaborated independently.*

And in harmony with this view stands the evidence adduced in Section III. The rule there formulated regarding the relation of binocular to uniocular brightness is an instance. Further, the difference between the sensual result of superposition of two similar images upon one and the same area of a single retina, and of placing them upon corresponding areas of the two retinae, could hardly be so great as it is, did apsychical or subsensual reactions underlying 'brightness' combine or interfere in the two retinal systems. If the binocular combination is a synthesis of a left-eye with a right-eye *sensation*, the difficulty disappears. Similarly, the 'prevalence of contours' in binocular vision, and the phenomena of 'retinal rivalry,' are explicable if each member of a pair of corresponding points yields a sensual entity which, when not widely dissimilar from that yielded by its twin point, fuses with that to a binocular sensation. In 'retinal rivalry' we have an involuntarily performed analysis of this sensual bicompound. The binocular perception in that case breaks down, leaving in phasic periods one or other of the simpler component sensations bare to inspection.

It is 'retinal rivalry' that in my judgment produces the marked difference in character of the flicker of the 'alternate' and 'synchronous' arrangements respectively at frequencies of intermission much below that required for extinction of their flicker. The 'alternate' arrangement then yields a flicker which though very marked is described by most observers as "less than" that of the 'synchronous' arrangement. It is "less decided" and "more irregular and hesitant," so that the observer is led to anticipate that as the frequency of intermission in both discs is increased flicker will disappear from the 'alternate' arrangement the earlier (*v. supra*, p. 34). 'Retinal rivalry' cannot of course occur in the 'synchronous' arrangement; but with slower and slower frequencies of intermission it becomes for the disc with 'alternate' arrangement more and more perceptible and obvious. Now 'retinal rivalry' is itself evidence of the physiologically independent development of two uniocular sensations. The difference therefore between the flicker given by the 'alternate' and 'synchronous' binocular discs under slow frequencies, argues, just as does their want of difference under higher frequencies, for physiological independence of the right and left component 'points' of 'corresponding' pairs.

Helmholtz in opposition to Panum[1] argued in favour of a purely psychical origin for 'prevalence of contours.' He invoked an explanatory 'direction of attention.' An inference he drew at the time regarding retinal rivalry accords with the inference drawn above from the flicker observations dealt with here, viz.[2]: "dass der Inhalt jedes einzelnen Sehfeldes, ohne durch organische Einrichtungen mit dem des anderen verschmolzen zu sein, zum Bewusstsein gelangt, und dass die Verschmelzung beider Sehfelder in ein gemeinsames Bild, wo sie vorkommt, also ein psychisches Act ist." A finely illustrative experiment on contours given by E. Hering[3] is applicable in the same sense.

McDougall[4], in applying to 'retinal rivalry' and 'prevalence of contours' his principle of competition of inter-related nerve-elements for energy, also argues a "separateness of the visual cortical areas for the two eyes." He brings forward striking experiments in evidence of this. In one of these he[5] shows that an after-image, left from excitation of one retina, is more strongly revived by subsequent weak diffuse excitation of that same retina than of its fellow. More recently, in experiments proving reinforcement of visual sensations by the activity of the ocular muscles, as evidenced by after-image observations, he[6] shows that activity of the intrinsic muscles of an eye sends up to the brain an influence, reinforcing the activity of the cerebro-retinal tract of that eye, while it exerts no such effect upon the corresponding tract of the other eye, or exerts it in a minor degree only.

With this separateness of the mechanisms, wherein are produced the sensations generated in the two retinae, the results recorded here from a different line of experimentation accord well. In certain respects the independence of the two mechanisms seems rather greater in regard to the tests applied in this inquiry than to those employed by McDougall. But the visual phenomena investigated in the two cases are not easily comparable. The chief evidence for some slight low-level communication between the right and left eye-systems, elicited by my work, has been the slightly lower frequency of stimulation found necessary for flicker-extinction under 'alternate left-right' excitation than under 'simultaneous.' The slight excess of brightness of the haploscopic over the uniocular image, recorded by so many observers, and shown by Piper[1] to be much more considerable for the dark-adapted eye, may be taken as evidence in the same direction.

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[1] *Physiologische Untersuch. üb. d. Sehen mit zwei Augen*, Kiel, 1858.

[2] *Physiologische Optik*, 2 Auf., Leipzig, 1896, S. 921.

[3] Hermann's *Handbuch d. Physiologie*, Bd. III. S. 384.

[4] "The Principle underlying Fechner's 'Paradoxical Experiment' and the Predominance of Contours in the struggle of the two Visual Fields." This Journ. p. 114.

[5] *Mind*, 1901, N.S. x. p. 56.

[6] *Mind*, 1903, N.S. XII. p. 473.

Binocular colour mixture may at first sight seem suggestive of a purely physiological fusion as the basis of binocular colour sensations. The facility of binocular colour mixture I find about the same when the uniocular colours are presented to 'corresponding points' by the 'simultaneous left-right,' or by the 'alternate left-right,' or by an 'intermediate arrangement' (Section I.). Of binocular colour mixture E. Hering writes[2]: "Hat man durch haploskopische Betrachtung zweier farbigen Flächen eine Mischfarbe erhalten und lässt dann genau dieselbe farbigen Lichtmengen auf eine und dieselbe Netzhautstelle fallen, so ergibt sich eine ungleich hellere oder weisslichere Mischfarbe." "Mischt man aber die beiden Farben binocular, so ist die resultirende Mischfarbe nur ungefähr gleich hell, wie die Einzelfarbe. Diese Thatsachen genügen schon, um selbst in den Fällen, wo die binoculare Mischung vollkommen gelingt, dieselbe der unocularen nicht gleichzustellen." With this conclusion my rule given above (page 50) is in complete agreement.

The compounding together of right and left images areally non-identical, but not widely dissimilar, is (Panum, Hering) the basis of visual 'relative depth-perception.' The compounding of visual images partly dissimilar—flickering with unflickering—seems a simpler case in the same category of synthetic actions. In the flicker experiments the visual components do not differ as to space-attributes and their combination has therefore no resultant differential space-attribute. But the synthesis gives in each case a compromise between the components in regard to the attribute wherein they do differ; in the flicker experiments, that is in regard to the sensual steadiness of the brightness. This amounts to the same as the rule formulated above for binocular combination of brightnesses of different intensities, but steady.

In these final considerations I have for the moment disregarded the evidence given in Section I. of minute, yet to most persons perceptible difference between the rates for flicker extinction under 'simultaneous' left-right and 'alternate' left-right stimulation described there. This difference may evidence a slight community or interference between the apsychical processes or subperceptual sensations derived from twin corresponding points, and a very slight low level intercommunication between their two systems. The slightly more perceptible flicker some persons obtain when observing an intermitting physical image binocularly instead of uniocularly, may likewise indicate the same very slight degree of intercommunication. The often recorded slight excess of brightness of an image seen binocularly as compared with one seen uniocularly may also depend on the same cause. But these appearances are too slight to practically invalidate the broad conclusion drawn in the preceding paragraphs.

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[1] *Zeits. f. Psychol. u. Physiologie d. Sinnesorgane*, xxxII. p. 161, July, 1903.

[2] Hermann's *Handbuch der Physiologie*, Bd. III. Tl. I. S. 596, 1879.

The above experiments on binocular flicker and brightness show that during binocular regard of an objective image each uniocular mechanism developes independently—at least as to steadiness of brightness and intensity of brightness—a sensual image of considerable completeness. The singleness of the binocular perception results from the combining of these elaborated uniocular sensations: it is the product therefore of a psychical synthesis that works with already elaborated sensations contemporaneously proceeding. Such synthesis lies obviously more within the province of study of the psychologist than of the physiologist.

A SIXTEENTH CENTURY PSYCHOLOGIST, BERNARDINO TELESIO.

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Telesio's De rerum natura: *its design to build up the Science of Nature on a new basis of empirical knowledge. The human spirit a part of nature, subject to natural laws, and identical in kind with the spirit of animals: its location in the brain and nervous system. Material interpretation of sensation. Empirical theory of space-perception, of memory and association, of reasoning. Sense-knowledge fundamental, and the source of all certainty. Naturalistic view of moral character.*

IN 1586 was published at Naples a work "*De rerum natura,*" in nine books, which still forms an interesting monument of patient study, tenacity of purpose, and consistency of thought. Its author, Bernardino Telesio, who at this date was verging upon eighty years of age, had left the University of Padua fifty years before, with a high reputation for scholarship, mathematics, and philosophy. The design was already formed in his mind of placing the philosophy—or science—of nature upon a new basis, and rebuilding the whole. For this end he had devoted himself during thirty years to a close study of the writings of Aristotle, and to an equally patient examination of nature by such methods as were then available. 'Nature' for him comprised the whole realm of human as well as of animal life and the inorganic world. In 1565 the first two books of his work—containing only the more general cosmological theories, although the whole was in fact completed—were published at Rome. In the same year "the good philosopher" was called to Naples, to expound and defend his theories: there and in his birthplace Cosenza, where he became the guiding mind of the Cosentine Academy, he lived for the remainder of his life, discussing, amending, and teaching the New Philosophy. He died two years after the publication of the completed work, and in the year following his death the youthful Campanella gave out his *Philosophia sensibus demonstrata*, which contained a defence of Telesio, "the greatest of philosophers," against his opponents[1].

Of the new theory of Nature with which the philosopher of Cosenza proposed to dislodge Aristotelianism from its hold upon the universities and the Church it must be admitted that it was greater in design than in actual accomplishment. Telesio's method was indeed beyond cavil;—he was to dismiss at one stroke from his mind all the dogmatic teachings of the schools, based as they were merely upon verbal authority, and to approach nature by a new path, through the one sure avenue of sense; for human wisdom would have reached its highest goal when it had studied all that sense revealed, and all that might be inferred by direct analogy from the things perceived by sense[2]. Like Bacon, he deprecated the arbitrary use of human conceptions in explanation of the natural world, and described his contemporaries as "anticipating nature (*veluti naturae praeeuntes*) and arrogating to themselves not only the wisdom but even the power of God[3]." Like the school of Bacon, he took as his one principle of inquiry that of the Uniformity of Nature,—"Nature is always in harmony with itself, is always the same, acts always in the same way, and produces always the same effects[4]." His conclusions, however, do not bear the same stamp of modernity upon them: they differ from the vague gropings after truth which were common enough with the innovators of the time only in the more frequent appeal to experience and to experiment, by which they are supported, and in the opposition to Aristotle which characterises them throughout.

The attack upon Aristotelianism is persistent and thorough, and no one had a better right to criticise the master than one who had spent so many years in the study of his works. Telesio proved, as he informed his patron the Duke of Nocera, that Aristotle's teaching was inconsistent with sense-knowledge, inconsistent with itself, and inconsistent with the Scriptures. In this last connexion his own attitude is curious, but it is one which has frequently been adopted since. In the introduction he proclaims his absolute acceptance of Holy Scripture and the decrees of the Catholic Church; whatever conflicts therewith, even sense-knowledge, is to be rejected at once. Spasmodically, throughout the work, he has occasional lapses into the consciousness that he is a Christian and a Catholic; we hear of a soul implanted in man directly by God, over and above the natural soul which grows with the seed of man, and lives in his body and there only: we hear again of an eternal life which belongs only to this God-given soul, and which is man's true end; but the life which we study in the work is the natural life of man upon the earth, bent, as are all other animals, upon his own self-preservation and perfection in natural gifts. These theological notes are invariably 'asides' from the real argument. The actual trend of Telesio's reasoning is shown by the fact, that in spite of his great popularity, the universal respect in which he was held, the friendship of many Popes and Cardinals which he enjoyed, only a very few years after his death his main work and his smaller tractates were placed upon the Index. In his outward life a model of orthodox humility, in his inward thought and in his works he passed far beyond the narrow circle of the Church. His creed was naturalism, of an extreme but singularly refined type: naturalistic in its analysis of the conditions of mental life, its view of the origin and validity of knowledge, its theory of the moral end and the practical virtues. A modern reader is amazed at the patience, the thoroughness, the calm seriousness with which Telesio works out his argument, and constructs the world, external nature and internal nature alike, on his new plan; these qualities are explicable only when we remember how many years he spent upon the work, how fully it was discussed and perhaps emended in the meetings of the Academy at Cosenza and of his disciples at Naples.

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[1] For a sketch of Telesio's work, the history of his school, and an estimate of his influence upon modern thought, v. Fiorentino's *Bernardino Telesio, ossia studii storici su l'idea della natura nel Risorgimento Italiano*, 2 vols., 1874. A full but uncritical account is to be found also in *Rixner und Siber's Beiträge*, vol. III. 1820.

[2] *Prooemium*, p. 1.

[3] *Ibid.*

[4] *Ibid.* p. 2.

Of his general theory of nature it is only necessary to say that for Aristotle's abstract principles of form and matter, possibility and actuality, Telesio substitutes two 'active' principles—heat and cold,—and one passive, the material or substrate, on and through which the former principles act. Heat is the constitutive principle of the sun and the heavens generally, cold that of the earth, and it is by the interaction of the two through the medium of matter, that individual beings, inanimate and animate, arise. The effect of heat is movement and rarefaction, that of cold immobility and condensation. Hence those bodies in which heat is the only or the predominant principle, as the sun or sky, are in constant motion, those in which cold predominates, as the earth itself, are motionless and dense. The action of heat shows itself in the brightness and whiteness of objects, its absence in darkness or blackness.

the body of the animal. "But other beings than animals,—since they are not composed of different parts (as body and spirit), or of parts one of which is covered by the other, but are all one and the same (homogeneous), and have throughout the same feeling and sense,—have not been endowed with *sensoria*....Moreover, unless in heat or cold or both there is sensation, and so in the heavens or the earth or both, neither can there be any in animals, which are constituted of these; for how can there be, in beings produced from the heavens and the earth, any faculty which is neither in the heavens nor in the earth[1]?" It is precisely the argument so familiar in modern "scientific philosophy," that if there is consciousness in the product—man—there must be consciousness in the elements of which the product is constituted,—the cell, the atom.[2] The movements of larger bodies are explained by the same principles of sense and desire. There remains, however, a certain inconsistency. For on the one hand motion is to Telesio the action—the outward side, as it were, the expression—of heat: heat is a substance, of which the necessary operation is motion, and heat is therefore "prior to motion in time, in nature, and in worth." Hence for example the constant motion of the sky (*coelum*); it cannot but move, and the nearer to it in nature any substance is, as spirit or air, the more mobile and restless that substance is. On the other hand, heat, cold, and the rest are said to move for the sake of an *ulterior end.* "Fire or any other thing changes its place, not because the place from which it moves is less akin to its own nature than that to which it goes; but because in the former it feels unpleasantness from some neighbouring thing, which is to be avoided; or because in the latter, there are things from contact with which it derives pleasure." The heat in the iron seeks union with the heat in the magnet, by which it feels it would be strengthened and preserved, therefore the weight of the iron is overcome, and it flies towards the magnet[3]. The only approach to a solution of the difficulty occurs in the fifth book[4], where it is said of the spirit or soul of man that it derives pleasure from sensations because they impel it to movement, that this is its proper action, and that by movement it is preserved in its true nature. Self-preservation is the end, motion the means by which the end is secured. But this would not apply to the active principle itself, which is always and of necessity in movement: motion is there an end in itself.

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[1] Bk. I. ch. 6, pp. 10, 11.

[2] Cf. Haeckel's *Monism*, pp. 19 and 41 of the English translation, and his earlier papers on "Cell-souls and Soul-cells," and "The Development of the Life Particle."

[3] Bk. IV. ch. 28, p. 166.

[4] Ch. 8.

The Psychology is contained in the fifth book, on the Spirit or Natural Soul in man, and in the seventh and eighth, on Sense and Intelligence respectively. As has been said, Telesio the theologian believed that there were two souls in man, one of which was evolved with the natural body, the other being introduced by God through a direct act of creation: the one concerned with seen and temporal things, the other with things that are unseen and eternal[1]. In this man is distinguished from all other creatures. "Even if this (natural) spirit is in man, and has in man the same function as in animals, and acts in the same way: it is yet not the substance of the human soul, nor does it of itself do all that man does; since Holy Scripture and Human Reason both teach us that there is another substance in man wholly divine and implanted by the Creator Himself[2]." The proof or evidence of this divine soul which Telesio offers is that men do in fact inquire into supernatural matters, which have no reference to their bodily needs, that they find real happiness only in the knowledge and pursuit of the divine; that for these they neglect even those bodily needs which the brutes pursue without deviation[3]. This divine soul is a concession which Telesio made to his theological contemporaries: the very terms he employs for it are scholastic—it is the *form* of the body, and especially of the spirit, &c.—and they remain unexplained, while nowhere else do such terms have any value for Telesio. The world beyond nature enters only at this one point into the course of nature. There is hardly any other suggestion of a transcendent principle in the work. Yet even so, the influence of the soul is limited to an extent which Telesio's friends in the Church could hardly have admitted. The divine soul is that in man which understands (*νοῦς ποιητικὸς*), but it does so only *through* the (natural) spirit, and it can understand only those things which the spirit offers to it for understanding[4]. Otherwise, if wholly independent of the natural spirit, "it would have no need to recall the perceptions of sense, nor to employ the reasoning powers, but would intuite not only all other (natural) beings, but itself, and those beings which cannot be perceived by sense,—the divine substance, and God Himself,—without effort, without succession of images, and without possibility of error, as other divine substances understand, including the freed soul itself when it has returned to God[1]." This intuitive knowledge, or creative intelligence, is that which all scholastic philosophy attributed to God, and other pure spirits, and Telesio was at one with it in this regard. But on that account he placed an impassable gulf between human and divine knowledge: the difference was not of degree, but of kind[2]. The supernatural soul was only added in man when the natural soul or spirit was already formed and perfected, and it had a separate sphere. Though it is with the latter alone that Telesio's philosophy is really concerned; there can be no question however of the genuineness of his belief in the supernatural: his was too honourable a character to profess a creed which he did not hold. Theology and science he kept rigidly asunder, and his psychology is as purely naturalistic as his theory of the universe is mechanical. Reasoning itself, as we shall find, he reduces to a 'mode of motion.'

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[1] *E.g.* Bk. I. ch. 8, p. 35 and Bk. II. ch. 25, p. 71.

[2] Bk. V. ch. 2.

[3] Cf. Bk. VIII. chs. 7 and 15.

[4] Bk. VIII. ch. 28, p. 349.

The spirit to which Telesio constantly refers as the natural soul, is thought of as wholly corporeal,—a very delicate, rarefied substance, enclosed within the nervous system, and therefore eluding our senses. Its place, the seat of the soul, is chiefly the brain, but extends also to the spinal cord, the nerves, arteries, veins, and the covering membranes of the internal organs. Similar cavities to those visible in the brain (*i.e.* the ventricles), the spinal cord and optic nerves, are present in all these organs, and it is there that the spirit is enclosed, so that it is accessible to any movement from without, and is able to transmit its own movement to these parts, and thence to the limbs[3]. The extreme mobility of the spirit, and its continuity throughout all the nervous system, are the qualities which fit it to play the part of the soul. By the contraction and expansion of the bodily parts (through heat or cold) the spirit is also contracted or expanded: this movement is transmitted to all other portions of it, and especially to the main portion of the spirit, in the cerebral ventricles: there sensation takes place. Telesio had followed closely the advances made since Aristotle in anatomy, and had studied in that science for himself, as his careful descriptions of the human eye and the human ear show. Certainly to locate the spirit in the ventricles of the brain was a step considerably in advance of the Peripatetic view that the heart, as the centre of the body, was the source of life and passion and desire. Recognising that the nervous system is in close connexion with soul-life, he frankly acknowledged that the soul

[1] Bk. VIII. ch. 6, p. 319.

[2] Bk. IV. ch. 25, p. 171.

[3] Bk. V. ch. 5, p. 181 ff.

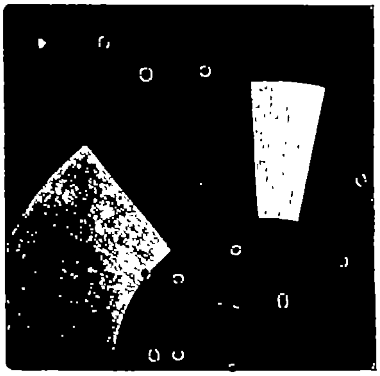

Fig. 3. In this figure and in Figures 4, 5, 6 and 8 the direction of rotation is from left to right or clockwise.

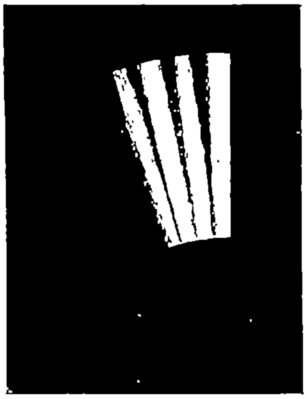

Fig. 4.

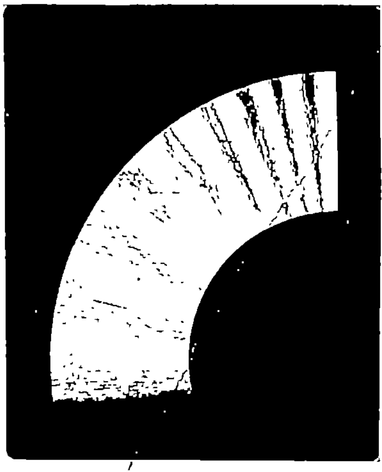

Fig. 5.

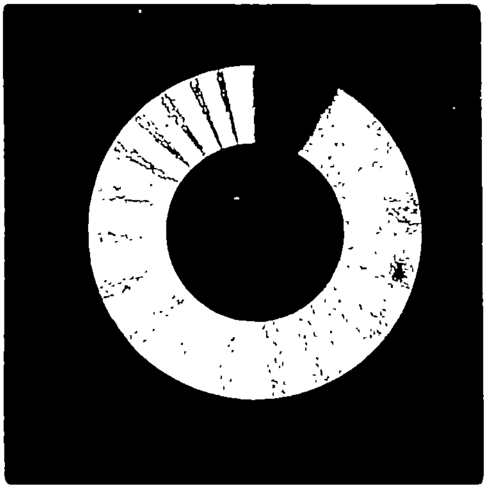

Fig. 6.

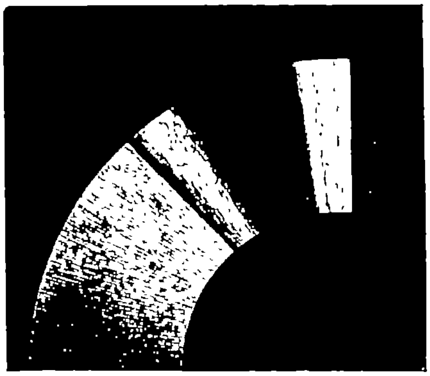

Fig. 8.

With this apparatus I have made a great number of observations, repeatedly studying the appearances under every combination of conditions. It may be stated at once that long-continued practice has greatly improved my power of directing my attention to peripheral parts of the field of vision while the eye remains at rest, and that no one who has had no such practice should expect to observe at once and with confidence all the appearances described below.

In making the following observations the eye was at a distance from the disc of about 1 metre and was kept steadily fixed upon the bead which was hung close to some part of the path of the image or about the centre of the disc.

A radial slit 2° in width and 7 cm. in length, its mid point 15 cm. from the centre of the disc, rotating at the rate of 1 rev. per 3″ before the glass lit by four burners, appears as a fanlike bundle of narrow bright rays of diminishing brightness from before backwards. Four such rays can usually be distinguished with certainty at this speed. They are not separated by distinct dark intervals but appear to overlap one another, and together they fill a sector about 12° in width. The bundle of rays is followed by a black space about 40° in width, and this by a dull white haze which fills the greater part of the path of the object-light. The appearance is represented with fair accuracy in Pl. I. Fig. 3. When the speed is a little increased the bright bands become more widely spread out and separated by narrow dark bands. This is the appearance (Pl. I. Fig. 4) described by Charpentier,—"on voit succéder des images doubles, triples, quadruples, separées par des intervalles sombres"[1]; and previously by Bidwell in 1894[2], and later in the following terms,—"The moving slit assumes the appearance of a fan-shaped luminous patch, the brightness of which diminishes with the distance from the leading edge. And if the eyes are steadily fixed upon the centre of the disc, it will be noticed that this bright image is streaked with a number of dark radial bands, suggestive of the ribs or sticks of a fan[3]."

When the speed of rotation is further increased to about 1 rev. per 1½″, the bands become more widely separated, wider individually, and more numerous, and the diminution of brightness in the series from before backwards becomes very marked.

The appearance is roughly indicated in Pl. I. Fig. 5.

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[1] *Arch. de Physiologie*, 1896, T. VIII. p. 677.

[2] *Proc. Roy. Soc.* Vol. LVI. p. 132.

[3] *Curiosities of Light and Sight*, p. 191. London, 1899.

With further increase of speed the bands become still more numerous, and at about 1 rev. per 1″, or at a rather higher speed, they fill nearly the whole circle. But with each increase of speed of rotation the time during which the light falls upon each retinal element is diminished, and this is equivalent to a diminution of intensity of the object-image; all the bands therefore lose correspondingly in brightness, and when they fill the whole circle the brightness of the whole is very much reduced so that the bands are obscure. In order to compensate for this effect of increased speed it is necessary to open the slit more widely. If then the slit is made 12° in width, and the disc rotated at 2 revs. per 1″, the whole circular path of the object-image appears filled with bright radial bands, separated from one another by darker bands, each bright band shading off on either side into a dark band (as in Pl. I. Fig. 6).

If now the brightness of the object-image is diminished in [illegible]ccessive stages, and the rotation repeated at each stage, with corresponding rates of rotation and width of slit, the leading bands are less bright and the whole series is less numerous, so that with light from two burners only, the bands do not fill the circle until the speed is about 2½ revs. per 1″; with one burner, not until speed is increased to about 3 revs. per 1″, and with one burner at half-power, not until the speed is raised to nearly 4 revs. per 1″.

The observation of the travelling object-image by an eye at rest yields, then, results concordant in every way with the effects described above as following on brief momentary stimulation of a small area of the retina by a stationary object-light. By both methods we find that a single momentary stimulus evokes from each retino-cerebral element a multiple response, a series of pulses of sensation (save in the case of stimulus of very low intensity which evokes a single response only); we find that the more intense is the stimulus the brighter are the initial pulses of the series and the longer is the series, and that the brighter are the pulses the briefer are they and the more rapidly do they succeed one another. By both methods we find that this primary response is closely followed by a secondary response in the shape of a steady duller image which dies slowly away and is brighter and more persistent the more intense is the stimulus. The facts of the primary response are represented in the accompanying diagram, Fig. 7.

The curves represent the series of pulses of sensation evoked by a single brief stimulus of fair intensity; the series begins at *a* at some unknown and undiscoverable interval after the moment of incidence of the stimulus, and lasts throughout a period of about ⅔ second represented by the length $a-i$. The height to which each curve rises represents the intensity of the pulse of sensation. The effects produced by a momentary stimulus of less intensity are represented by a part of the series of curves; thus the effects of a stimulus of intensity sufficient to give sensation of brightness indicated by the height of curve *e*, are represented by the curve *e* and by the series of curves from *e* to *i*, while a stimulus strong enough to give sensation of intensity indicated by height of *h* would be followed by two pulses of consciousness represented by the two curves *h* and *i*.

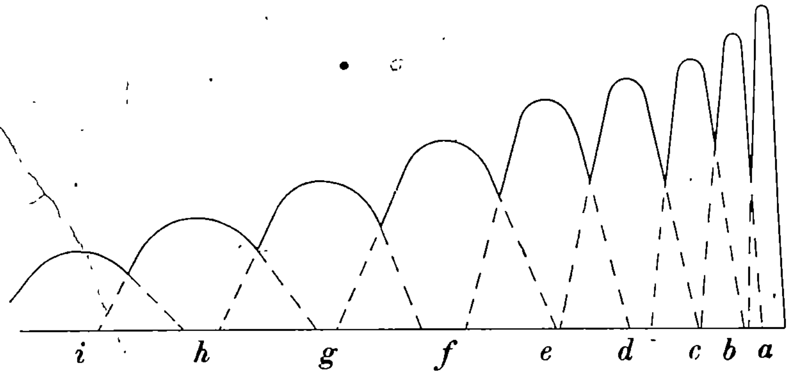

Fig. 7. Diagram illustrating the series of pulses of sensation, the primary response excited by a single momentary stimulus applied to any part of the retina.

The curves in the diagram are drawn overlapping one another. In this way I have sought to represent the fact that the successive pulses of sensation are usually separated from one another by intervals that are not completely dark. There seems in fact to be an overlapping, each pulse beginning before its predecessor has quite passed away.

The overlapping is greater the slower the speed. Thus in the case of a radial slit 2° wide rotating at 1 rev. per 3″ before the plate lit by four burners, the bright bands are not separated by darker intervals and are only distinguishable through being of different brightness. This appearance is represented in Pl. I. Fig. 3.

When the rate of rotation is increased to a little more than 1 rev. per 2″, the bright bands overlap to a less extent, and are separated by narrow darker intervals as in Fig. 4, the separation being most distinct at their outer ends where the rate of movement is greatest. With further increase of rate of rotation the dark intervals become still wider. If then the rate is gradually diminished, the bands close up again and overlap more and more like the bars of an ivory fan, until when the rate is only about 1 rev. per 5″ the image appears as a single homogeneous streak of bright light, not appreciably wider than the object-light. This strongly suggests that at this slow rate the image consists, in a sense, of the whole series of pulses of sensation succeeding one another so rapidly that the whole series runs its course before the image has moved to an appreciably different position, and so rapidly that they become continuous for consciousness.

These observations strongly support the view of the nature of the physiological processes underlying the visual sensations which I have put forward in a previous paper at some length[1], and which may be briefly summarised as follows:—The stimulus applied to the sensory end-organs, the rods and cones of the retina, determines in the afferent neurones of the retino-cerebral tract a liberation of energy. This energy which is possibly of a kind peculiar to the nervous system, we can at present only describe in terms of its behaviour in the nervous system, and for the sake of clearness and brevity I have proposed to speak of it as 'neurin.' Neurin seeks to escape from the stimulated afferent neurones, the place of high potential, to motor neurones and the muscles, places of low potential. In thus escaping it has to pass through the synapses of the visual cortex, the places of highest resistance on the sensory-motor paths. This resistance is overcome intermittently; the neurin escapes from afferent to efferent neurone across the cortical synapse in a rapid series of discharges, which are more voluminous and succeed one another more rapidly the higher the potential of the charge in the afferent neurones. Each such discharge determines a single pulse of sensation of which the intensity is in some way a function of the quantity of energy transmitted across the synapse in that discharge. When the stimulus is continuously applied to the retinal ends of the sensory-motor chain of neurones the synaptic discharges are usually repeated at such a rate that the sensation appears continuous; but when the stimulus is a momentary one only, it liberates in the afferent neurones a charge of neurin which escapes by a series of discharges of diminishing volume and diminishing rapidity, so determining the series of pulses of sensation which we have studied above[2].

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[1] *Brain*, 1901, Vol. xxiv. p. 577.

[2] This series of pulses of sensation provoked by a single brief excitation of the retina seems to me to present an interesting parallel to the series of contractions of skeletal

One important feature of the primary response of the eye to the momentary stimulus has yet to be described. We saw that as the rate of rotation of the narrow bright object-image is increased, the number of bright bands distinguishable, which is at first only three or four, becomes largely increased. One source of this increase, which has already been pointed out, is the spreading out of bands, which at the lower rate are so much overlapped that they form the single leading band of highest intensity. But there is a second source of this increase in the number of bands. As the rate of rotation increases there become visible several dim grey bands in the dark interval which separates the bundle of bright bands from the secondary response, the dull grey haze which fills the track of the object-light (see Figure 3). These are dimmer and broader than the leading bands and differ markedly in quality, the leading bands being yellowish-white, these others a neutral grey. This difference of quality is more marked if the object-light be made green (or blue). The bright leading bands are then green (or blue), while these later bands are again a neutral grey. The leading bright bands are in fact reactions of the colour-vision apparatus of which the cones are the retinal end-organs, while the later series of grey bands are reactions of the apparatus for vision in dim light of which the rods are the retinal end-organs. They may then be conveniently distinguished as cone-bands and rod-bands respectively. This view of their nature is borne out by the following facts. Firstly, a homochromatic red object-light produces a series of red bands only, corresponding in number and distribution to the leading coloured bands given by a green or blue object-light, and the rod-bands are brightest when the object-light is green; we know from the researches of v. Kries and others that the rods are not stimulated by pure red light, but are stimulated by all other kinds of light, and most effectually by green light. Secondly, if the fixation-point is put in such a position that the outer end of the green object-light passes behind it at one point of its path, the grey rod-bands appear to have their outer ends broken off at the moment they pass the fixation-point and restored in the succeeding moment, *i.e.* this later part of the series of responses is not given by the fovea where rods are absent. Thirdly, these rod-bands have that peculiar neutral ghostly quality, inclining perhaps slightly to blue, which I have learnt to recognise with some confidence as the

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muscles that is evoked in a strychnized frog by a single excitation of the cutaneous surface. (Cp. *Proceedings of the Physiological Society*, July 19th, 1902. Note by J. Burdon-Sanderson and F. Buchanan, *Journal of Physiology*, Vol. xxviii.)

quality characteristic of sensations due to the rod-apparatus. Fourthly, these rod-bands never appear when the eye is thoroughly light-adapted, and the sensitivity of the rods thereby reduced to a very low degree, or abolished.

I shall shew below that the beginning of the sensation evoked by stimulation of the cones follows the incidence of the stimulus at a shorter interval than that which intervenes between the stimulation of the rods and the resulting sensation, but that this relative delay of the rod-reaction amounts to $\frac{1}{18}$ second only. The interval which separates the rod-bands from the leading cone-band in the above-described series of pulses of sensation is much greater than $\frac{1}{18}$ second, and their appearance after the cone-bands is therefore not simply due to this more sluggish reaction of the rod-apparatus. It is, I think, rather to be explained by the greater sensibility of the rods in the dark-adapted eye. The stimulus which evokes a series of responses of a certain length from the cone-apparatus, produces a longer series of responses from the more sensitive rod-apparatus, and while the earlier of these rod-responses are overlaid and obscured by the more vivid cone-responses, the later members of the series appear as the dim grey rod-bands following the cone-bands, as described above. The non-appearance of the rod-bands during the slower rates of rotation of the bright object-light will be explained below in the section dealing with 'Bidwell's ghost.'

The Secondary Response.

Before going on to deal with this, I must say a few words about the secondary response which follows the pulsating primary image. This secondary image is much less bright and much more persistent than the primary image; it appears abruptly and dies slowly and steadily away. In the case of momentary stimulation by a bright stationary object-light, it seems to appear at a great distance at the moment in which the pulsating image comes to an end, and rushing up towards the eye to stop suddenly, filling the position of the primary image. This impression of rapid approach is, I think, due to its beginning in the centre of the area and spreading rapidly outwards, the whole process occupying only a small fraction of a second. This grey image then fades slowly and so gradually that the moment of its disappearance cannot be fixed. But it is brighter and of longer duration the brighter the object-image.

In the case of the travelling object-image the secondary image appears abruptly at a brief interval after the last of the bands of the primary image, and fills a part or the whole of the track of the image. In both cases, if the object-image is coloured and of high intensity the secondary image is tinged with the same colour; but if the object-image is coloured and of low intensity the secondary image is grey in case of blue and green light, but is absent in the case of pure red light.

This secondary image is simply the ordinary persistent after-image. In the case of an object-light of low intensity, it is due to the more sensitive rods alone, as shewn by its grey quality and its absence in the case of pure red light. In the case of a brighter object-image it is a combined rod- and cone-effect, the cone-effect preponderating the more the brighter is the object-image, as shewn by the greater saturation of the colour of the secondary image. In a previous paper[1] I have tried to shew that the ordinary after-image is in all cases primarily due to the continued action in the rods and cones of exciting substances set free in them from stored mother-substances (visual purple in the case of rods, similar, but hitherto unknown substances, in case of cones) by the action of light. This view is borne out by the following very constant feature of the secondary image or after-image which follows the primary pulsating image of the travelling object-light. When the object-image is a narrow band of only moderate intensity, the secondary image persists about one or two seconds only, so that at the rate of 1 rev. per 3″ it fills only about one-half of the circular path of the object-image. If then the eye remains fixed upon one point in the neighbourhood of this path (best the centre of the disc) the secondary image becomes brighter and longer drawn out with each revolution of the object-light until it fills the circle. Further revolutions cause it to become more and more bright so that it interferes seriously with the observation of the primary image, and if then the eye is closed it persists as a circle of light which is bright and persistent in proportion to the number of revolutions of the disc during which the fixation has been maintained. This effect is thoroughly concordant with my view of the nature of the after-image; for at each passage of the light over any part of its path upon the retina it seems to liberate a fresh quantity of the exciting substances, which therefore, accumulating with each revolution, determine an increasingly bright and persistent after-image.

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[1] "Some new Observations, etc." *Mind*, 1901, N. S. Vol. x. p. 52 etc.

I regard, then, the primary pulsating response as due to the activity of the afferent neurones of the sensory-motor arc excited by the very brief but intense photo-chemical processes that occur in the rods and cones during the moment of the action of the light upon them, this activity being continued for a brief period beyond the moment of the incidence of the light; while I regard the secondary response or persistent after-image as due to the continued feeble stimulation of the endings of the optic nerve by the substances set free in the rods and cones by the action of the light, and not entirely used up during that action.

Among authors who have recognised the fact that the primary response of the eye outlasts the brief stimulus, Parinaud[1] alone, so far as I am aware, has sought to distinguish the seat of this continued activity from the seat of that which determines the enduring after-image. But curiously enough he regards the former as retinal and the latter as cerebral. He names the primary response "l'impression rétinienne persistante," and describes it as a "sensation lumineuse tremblotante," while the secondary response, the ordinary after-image, he names "l'image consecutive positive," assigning it a cerebral seat on grounds the fallaciousness of which has been frequently exposed, namely, the facts of the projection of the after-image of one eye on a surface presented to the other eye, and the movements of an after-image corresponding to those of the eye.

Of the authors who have observed the multiple character of the primary response, C. A. Young proposed a theory which may be best stated in his own words:—"The phenomenon at least suggests the idea of a reflection of the nervous impulse at the nerve extremities—as if the intense impression upon the retina, after being the first time propagated to the brain, were then reflected, returned to the retina, and, travelling again from the retina to the brain, renewed the sensation[2]."

Davis, who worked by both methods, that of the stationary and that of the travelling image, and who noted the absence of the ghost in the case of pure red light, gave the name 'recurrent image' to the phenomena observed by both methods, and attempted to explain them by the suggestion that when the retina is stimulated by light of one colour, the 'nerve-current' excited in the nerve-fibres subserving the sensations of that colour induces currents in the nerve-fibres subserving the sensations of other colours, much as an electrical current in one wire induces a current in neighbouring wires[1].

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[1] *La Vision*, Paris, 1898.

[2] *Loc. cit.*

Bidwell has suggested that the 'ghost' is due to a delayed reaction of the 'violet nerve-fibres,' but has not attempted to explain the multiple bands seen by him in the case of a bright moving slit, or the series of pulses of sensation excited by a brief stationary object-image.

Charpentier proposes to explain the multiple response in the case of the travelling image by a peculiar theory of retinal oscillations; he supposes that the outer ends of the rods and cones are thrown into transverse vibrations, and that waves of such movements pass across the retina, polarised, as he says, in two directions[2]. It is, I think, very difficult to suppose that the rods and cones, embedded as they are in the pigment epithelium, can be subject to any such vibrations, and I confess that I fail to understand how such oscillations, if they did occur, should produce the effect of a multiple response, a series of pulses of sensation. The hypothesis has the further drawback that, even if we should accept it as capable of explaining the effect in the case of the travelling image, it would seem to be inapplicable to the explanation of the similar effect following momentary stimulation by a stationary object-image.

'Bidwell's Ghost.'

The 'recurrent image' or 'Bidwell's ghost' in its typical or most usual and easily seen form may be observed by the following procedure. A narrow slit (from 2° to 5° in width) in the large disc rotates at 1 rev. per 3″ before the plate of milk-glass, moderately brightly lit (by rather less than the full power of one burner, in the place of the four burners used in the experiments described above, p. 91). The eye having been made thoroughly bright-adapted, is kept covered for about three minutes and then fixed upon any point in or near the path of the moving slit. There then appears (see Pl. I. Fig. 8) a bright leading image (which, as the practised eye can readily discern, is made up of several partially overlapping radial bands) followed after a dark interval of about 25° by a dim grey image, in the form of a narrow radial band. The two images differ not only in brightness but in quality, the leading image (or bundle of images) is of the colour of the object-light, the 'ghost' is grey, or, at most, faintly tinged with colour.

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[1] *La Vision*, Paris, 1898.

[2] *Archiv de Physiologie*, 1896, T. VIII. p. 677.

This after-following image, which is the commonly described 'recurrent image,' the typical 'ghost' of Bidwell, is merely the last of the series of rod-bands described above, the last of that series of pulses of sensation which constitute the full primary response of the visual organ to the momentary stimulus, the intermediate bands of the series being inhibited by the bright leading bands. As I shall shew below, the 'ghost' is not in every case a pure rod-band, but it is always merely the last of the series of pulses of sensation excited by the moving object-light, the gap in the series being due to inhibition of intermediate pulses by the initial most vivid pulses. This proposition I must proceed to justify.

In the first place, this typical 'ghost' which follows an object-light of moderate intensity is shewn to be a reaction of the rod-apparatus by the facts, (1) that it jumps the fovea (this fact v. Kries[1] has carefully studied, and I can confirm his statement); (2) that it never appears in the case of a pure red object-light; (3) that it is relatively brightest with green object-light; (4) that it has commonly the peculiar neutral quality of rod-sensations (in some cases it appears tinged with colour, a fact to which I shall recur below).

That this 'ghost' is but one of the series of rod-bands described above may be shewn as follows:—A slit about 3° wide (white, green or blue) rotates at 1 rev. per 3″, and the illumination is so adjusted that the moderately dark-adapted eye sees the typical 'ghost.' The light is then gradually diminished, when a second 'ghost,' similar to the first, but dimmer, begins to be visible in the dark gap between the leading bright bands and the first 'ghost,' and as the light is further diminished this becomes equally bright with the original ghost, or if the rate of movement be a little greater, two such additional 'ghosts' appear, filling the gap between the bright cone-bands and the original 'ghost.' When the light is still further diminished these rod-bands begin to preponderate over the cone-bands, and a stage is reached at which the whole reaction consists of a series of five or six dim rod-bands. These are so dim and delicate that it is essential that the field on which the object-light moves should be absolutely dark.

Or again, if instead of diminishing the intensity of the object-light, and so diminishing the inhibitory power of the leading cone-bands, the rate of rotation of the disc is gradually increased, a similar effect is obtained, the inhibited intermediate rod-bands become visible, filling the gap between the cone-bands and the 'ghost.' The increased rate of rotation diminishes the inhibitory power of the leading bands in two ways; in the first place they are rendered less bright because the stimulus acts for a shorter time on each retinal element in its path, and secondly, the whole series of bands is more widely spread out on the retina, and we know that the inhibitory power of one image on another diminishes rapidly with increase of separation of the two on the retina.

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[1] *Zeits. für Psychol. u. Physiol. d. Sinnesorgane*, 1896, Bd. XII. S. 84.

We may reverse both these procedures, and we then find the reverse effects. Thus, if while the typical single 'ghost' is following the leading bands, the intensity of the illumination, *i.e.* of the object-light, is gradually raised, the 'ghost' becomes dimmer and then altogether invisible. The appearance represented in Fig. 8 gives way to that of Fig. 3; the 'ghost' is completely inhibited by the brighter cone-bands. (Bidwell and Charpentier in observing the multiple images of a moving object-light clearly used a light so bright that the leading cone-bands inhibited all the rod-bands, for the banded character of the image is most readily seen in the case of a narrow object-light of high intensity.)

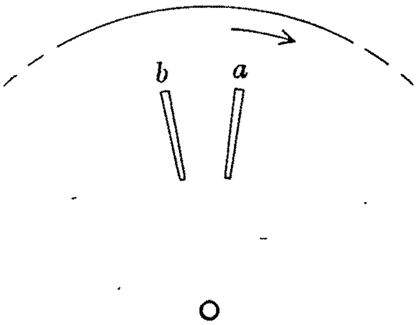

FIG. 9.

Again, if the object-light be made of such intensity that, while moving at a rate of 1 rev. per 2″, it gives the appearance of the typical 'ghost' following the leading cone-bands at an interval of about 35°, and the rate of movement is then gradually diminished, the interval between the cone-bands and the 'ghost' is correspondingly diminished, and as the 'ghost' approaches the bright cone-bands it becomes dimmer, until, when the interval is reduced to about 20° it becomes altogether invisible, *i.e.* it is completely inhibited.

That the initial images or reactions provoked by the travelling object-light really exert a strong inhibitory influence upon the retino-cerebral elements over which the image has passed in the preceding moments, and that the gap in the series of images is due to this influence, is proved by the following experiments:—Two radial slits 3°

wide and 7 cm. long are made in the disc at an angular interval of 20° as in Fig. 9, so that on rotation the slit *b* follows in the path of slit *a*. Then, on rotation at 1 rev. per 3″ before the brightly lit surface, *b* appears much less bright than *a*; and if then a sheet of white paper is pasted over *b*, so as to reduce the brightness of that slit by rather more than 50%, and the rotation repeated, *b* is quite invisible so long as the eye remains at rest. That this effect is due not to inhibition alone, but also to fatigue, is shewn by reversing the direction of rotation, when *b* is visible[1].

I found that as a lecture-demonstration this experiment is made more striking if *b* is made red (or of other colour) while *a* is white. Then while the disc rotates in direction of the arrow in the figure and the eye remains at rest, no trace of the red image can be seen, though it becomes visible immediately if the eye is allowed to follow the moving image. This variety of the experiment has an interesting bearing upon theories of the visual processes because it proves that a white image produces fatigue for a coloured image, and also exerts an inhibitory effect upon a coloured image, facts which, as I have previously pointed out, seem to be inexplicable by Hering's theory of opponent colour-processes.

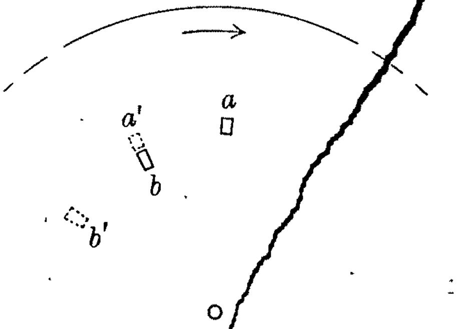

Fig. 10.

That the 'ghost' is easily inhibited by other brighter images is also proved by other experiments, of which the following may serve as an example:—Two radial small slits, *a* and *b*, 3° wide and 1 cm. long, are made in the disc at an angular interval of 30°, and in such a way that the path of the one lies immediately outside the path of the other (as in Fig. 10). Then one of them, *b*, is covered over and the illumination and the rate of rotation are so adjusted that the other one,

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[1] This inhibitory phenomenon falls readily into the scheme of the inhibitory processes which I have proposed (*Brain*, 1903, vol. xxvi. p. 153). According to that scheme the most intensely excited chain of retino-cerebral elements forms the path of least resistance of the whole retino-cerebral tract and therefore drains to itself some or all of the energy liberated in other parts of the tract, this drainage being more effective in proportion to the proximity of the two chains of elements concerned.

a, gives the typical 'ghost' (the rotation being in the direction of the arrow), following the leading image at an interval of 30°. On uncovering *b* and repeating rotation at the same rate and in the same direction as before, *b* is followed by a 'ghost' at an interval of 30°, while no 'ghost' is visible following *a*; and on rotating the disc in the opposite direction, but at the same rate as before, *a* is followed by a 'ghost' and *b* has none; in each case the 'ghost' of the leading image is inhibited by the other bright image upon the immediately adjoining part of the retina.

The typical 'Bidwell's ghost' is, then, a reaction of the rod-apparatus, as v. Kries has suggested[1]. But in making that suggestion v. Kries believed that we have a single cone-reaction, followed by a single rod-reaction which is normally about $\frac{1}{5}$ sec. slower than the cone-reaction. Now I shall shew below that the normal rod-reaction begins only about $\frac{1}{18}$ sec. later than the cone-reaction, and I have to shew also that a pure red light may be followed by a 'ghost' (a fact denied by v. Kries and Bidwell) and that the 'ghost' is therefore not in all cases a rod-reaction.

The Delay of the Reaction of the Rods.

We have no means of determining the length of the interval which intervenes between the moment at which light falls upon the retina and the moment of the beginning of the resulting sensation[2], but we can compare the duration of this period in the case of sensations of different quality and intensity. The visual reaction-time exceeds the auditory reaction-time by about 40σ, and in all probability this represents the time occupied by the chemical changes in the retina. Now since the chemical changes produced in the rods are different from those in the cones, and since the rods represent the more primitive, and the cones the more recently and more highly developed, parts of the visual organ, we may fairly expect to find that the photo-chemical changes of the cones occur more rapidly than those of the rods, just as the modern photographic plate excels the more primitive plates of thirty years ago in the rapidity of its reaction to light. And this we find to be the case; we find that sensations due to the cones are more rapidly developed than those due to the rods. If a slit 20° wide and transmitting light of any colour, other than pure red, and of low intensity, is rotated at 1 rev. per 3″, and the dark-adapted eye is fixed upon any point near the path of this object-light, the image appears as a band of colour rather more than 20° in width, and the colour of the leading part of this band is much more saturated than the remainder of it, which, if the intensity of the object-light is low and the eye thoroughly dark-adapted, may appear almost colourless. V. Kries has described this appearance, and has rightly ascribed it to the more sluggish reaction of the rods, and he has roughly estimated the delay of the rod-reaction as compared with the cone-reaction, at about $\frac{1}{50}$ sec. or 20 σ[1].

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[1] "Ueber die Funktion der Netzhautstäbchen," *Zeits. für Psychol. u. Physiol. d. Sinnesorgane*, 1895, Bd. IX. S. 81.

[2] Charpentier ("Durée de l'excitation latente de l'appareil visuel," *Compte Rendu d. Soc. d. Biologie*, 1888) claims to have measured this interval. He shews that, if three similar flashes of light be thrown in rapid succession upon an area of the retina, the interval between the first and second must be greater than that between the second and third by about 20σ in order that both intervals shall be appreciable. From this he infers that the interval between the first stimulus and the first sensation is 20σ greater than that between the second stimulus and the second sensation. Even if we accept this questionable inference we shall not be justified in accepting the figure 20σ as a measure of the interval in question; it is at best merely the measure of the difference of the intervals in the two cases. The interval between stimulus and sensation may be indefinitely large, as much as one second, one minute or even a year, and, if it is the same for all kinds of sensation, I do not think that we shall ever be able to discover the fact; though in all probability it is less than the time of a simple sensory reaction.

It is easy to measure the amount of this delay with fair accuracy, by taking advantage of the well-known fact that the eye can appreciate with great delicacy any departure from the same straight line of two straight lines lying end to end. It is necessary, in the first place, to answer the questions, Do two cone-sensations of different quality develope at different rates? and secondly, Do cone-sensations of similar quality but different intensity develope at different rates[2]?

A radial slit 2° wide and 10 cm. long, its inner end being at 10 cm. from the centre of the disc, is covered in its outer half with several layers of red gelatine so chosen as to transmit very nearly pure red light; the central half of the slit is covered with layers of blue gelatine until the two halves appear to be about equally bright, though differing in colour. The disc is rotated before the brightly lit surface at a rate of 1 rev. in 1½″, while the eye is fixed upon any point near the path of the slit. The two halves of the slit then shew no departure from the same straight line. If one of these two sensations, the red or the blue, developed more rapidly than the other, the leading edge of the image of that colour would appear to precede that of the image of the other colour, even if the difference amounted to no more than 4 σ (for I find that the eye can detect with certainty a difference of less than 1°). Comparing in this way the rates of development of red, green, blue, and white sensations I find no appreciable difference so long as the two parts of the slit are made of approximately equal brightness[1].

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[1] "Ueber die Wirkung kurzdauernder Lichtreize auf das Sehorgan," *Zeits. für Psychol. u. Physiol. d. Sinnesorgane*, 1896, Vol. XII. S. 97.

[2] The papers by Exner "Die zur Gesichtswahrnehmung nöthige Zeit" (*Ber. der k. Akad. Wien*, 1868) and by Kunkel (*Arch. f. d. ges. Physiol.* 1874) which are sometimes referred to as giving answers to these two questions, really deal with closely allied but different problems which I hope to treat in a separate paper.

If now the two halves of the slit are made of similar quality, but of very different intensity, by covering the one half with layers of tracing-paper sufficient to cut off about 80 °/₀ of the transmitted light, and if the reaction of the rods is excluded by using pure red light or by making the observation with a very thoroughly light-adapted eye, then, on rotation at 1 rev. per 1½″, the image of the brighter half of the slit begins more abruptly and its leading edge precedes that of the duller half by about 2°, and to a proportionally greater extent if the rate of revolution is increased. The rate of development of the dull and the bright sensations is then not quite the same, and, in the case of this very considerable difference of brightness, the difference of rate of development is about 8 σ in favour of the brighter sensation[2].

A slit of this kind, of which both halves transmit green light, the one half being made considerably brighter than the other, serves to shew the lagging of the rod-sensation, in the following way:—The slit is rotated at 1 rev. per 1½″ before the plate of milk-glass, lit with full power of four burners, and observed with thoroughly dark-adapted eye fixed upon any point. The slight lagging of the duller half amounting to about 2° is then perceptible, and as the illumination is gradually diminished this amount of lagging of the duller half persists so long as the duller half remains distinctly green in colour. But when the illumination is so far reduced that the duller half appears colourless to the dark-adapted eye (and is invisible to the light-adapted eye), the lagging of this duller half suddenly increases to about 12°.

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[1] Charpentier (*Arch. de Physiologie*, 1898, "Différence de Temps perdu suivant les couleurs") finds, by experiments similar to those described above, that the development of the sensation of red is more rapid than that of green by some 3 σ to 6 σ and than that of blue by twice this amount. I can only suppose that individuals differ in this respect, for since reading this paper of Charpentier's I have repeated the comparison of blue and red very carefully and can detect no difference. If in my case so large a difference as 6 σ existed I should certainly detect it.

[2] This fact suggests the possibility that the sluggish development of blue sensation observed by Charpentier may have been due to his using a blue object-image considerably less bright than the red image. Since he used lamplight transmitted by coloured glasses, this would necessarily be the case unless special precautions were taken.

It is important to note that a similar sudden increase in the lagging of the duller half occurs when the two halves of the slit are white, or of any colour but pure red.

This lagging of the rod-reaction may be displayed by many modifications of this experiment, but the extent of the lagging may be best measured as follows:—Two slits, each 5 cm. long and 2° wide, are made, lying on radii of the disc at an interval of 10°, and in such a way that the path of the one is immediately outside that of the other, as in Fig. 11.

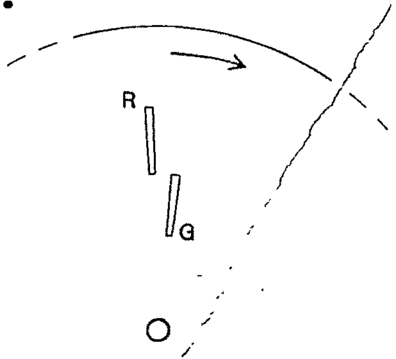

Fig. 11.

The one slit *R* is covered with several layers of red gelatine, so chosen as to transmit almost pure red light; the other slit *G* is covered with layers of green gelatine, until to the light-adapted eye it appears by transmitted light rather less bright than the red slit. The illumination of the glass plate is then lowered until the green slit appears colourless to the thoroughly dark-adapted eye. On rotation of the disc in the direction of the arrow in the figure (*i.e.* so that *G* leads), the image of slit *G* falls back relatively to the image of slit *R*, so long as the eye remains at rest, and if the rate of rotation be as much as 1 rev. per 1¼″, *G* appears actually to follow after *R* at a considerable interval. The rate of movement at which the leading edges of the two slits appear to fall along the same radius may then be found by varying the rate of rotation. This rate is, for my eye, as nearly as possible 1 rev. per 2″; that is to say, the rod-image *G* lags, relatively to the cone-image *R*, 10° when the rate of movement is 1 rev. per 2″[1]. The amount of this delay seems to be about the same for different individuals, for some dozen persons, to whom I have demonstrated this experiment, see the two images fall into line at the same rate of revolution of the disc. We may conclude, then, that the rate of development of the cone-sensations exceeds that of the rod-sensations by about 55 σ or $\frac{1}{18}$ second.

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[1] In this experiment the lagging of the colourless sensation excited by the green object-image is not in any degree due to a difference of intensity of the two sensations, for to the thoroughly dark-adapted eye the colourless rod-sensation is distinctly brighter than the red sensation.

In the above descriptions I have assumed that the colourless image of the green object-image of low intensity seen by the dark-adapted eye is a sensation excited by the rod-apparatus or apparatus for vision in a dim-light, accepting as established that view of the function of the rods which we owe to Max Schultze and to Parinaud, and which has been presented in the most satisfactory form by v. Kries. That view is, I think, abundantly justified, and the experiments last described seem to me to constitute one of the strongest pieces of evidence in support of it. It is considered by some a grave objection to this view that it involves the acceptance of white or neutral-toned visual sensations of two different physiological origins, the primitive neutral sensations yielded by the rods and the 'trichromatic white' resulting from psychical fusion of the elementary colour-sensations. The fact demonstrated above, that of the physiological processes underlying the two grades of colourless sensation, those underlying the dim vision of the dark-adapted eye require for their completion as much as 55 σ longer than the processes underlying the white sensations of the bright-adapted eye, while the latter run their course in exactly the same time as the processes underlying the colour-sensations, this fact goes far, I think, to compel us to accept v. Kries' view of the function of the rods.

The 'ghost' of a pure red light.

V. Kries and Bidwell have stated that no 'ghost' can be obtained with a pure red light, but this, like most of the other misunderstandings of this subject, arises from an insufficiently wide variation of the conditions of observation, as is shewn by the following experiment. A radial slit, 3° wide and 10 cm. long, is covered with layers of red gelatine until, on inspection through the spectroscope, it is found to transmit only red light. This, when made of about the same brightness as the white, green or blue object-light that gives a typical single ghost, gives of course no rod-bands, and therefore no 'ghost.' It appears as a bundle of closely set red cone-bands only. But if the intensity of this red object-light is much increased there appears, at a certain degree of brightness and at a moderate speed of rotation, a narrow bundle of bright red bands followed after a dark interval of about 25° or more (according to the speed) by a single narrow red band of low intensity but of good saturation, forming a 'ghost' very similar in general appearance and behaviour (except as regards colour) to the typical 'Bidwell's ghost.' The only difficulty in demonstrating this red ghost is that of securing a sufficiently intense source of light, for the plate of milk-glass used in the preceding experiments, together with the five or six layers of red gelatine, cuts off so much of the light that my acetylene lamp proved insufficiently powerful. This difficulty I have overcome by using direct sunlight, and I have also been able to demonstrate the red 'ghost' as a lecture-experiment by using a plate of ground-glass as the background for the rotating disc, and focussing on to it from behind the direct ray of an arc lamp, while the observers sit directly in front of the disc.

There can be no doubt that this red 'ghost' is merely the last of the long series of pulses of sensation evoked by the intense red object-light, the intermediate bands of the series being inhibited by the very bright leading bands; for by careful adjustment of the brightness of the object-light, and by varying the rate of its movement, the whole long series of red cone-bands may be made visible.

Other kinds of coloured 'ghost.'

With lights of other colours the 'ghost' is not always colourless, *i.e.* it is not always a pure rod-reaction. Some observers have described it as being of the same colour as the object-light, others as being of the complementary colour. In the case of a blue object-light of moderate intensity I have frequently seen the 'ghost' distinctly tinged with blue, and rarely I have seen it yellowish in tone. In the case of a green object-light I have frequently seen the 'ghost' tinged with the complementary purple, rarely I have seen it greenish in tone. These colours have been in all cases of very low saturation, and their appearance depends upon so complex a conjunction of conditions that it is difficult to define those conditions with any accuracy. But in general terms it may be said that these tinges of colour are apt to appear in the 'ghost' when the eye is but very little dark-adapted, and the colour of the object-light is of low saturation, and that the complementary colour is especially apt to appear when the object-light is made so bright that any further increase of brightness produces total inhibition of the 'ghost.' These faintly coloured 'ghosts' seem to be compounds of rod-reaction and cone-reaction in which the former predominates, that is to say, these late rod- and cone-reactions are synchronised so as to fall in the same moment and appear as superposed or mixed bands, hence the colour; and the complementary tinge which sometimes appears is due to the stronger inhibitory effect of the leading coloured bands upon the constituent of the compound which is of its own quality, *i.e.* the complementary colour is a contrast effect.

That there obtains a synchronisation of the reactions of the retino-cerebral elements subserving the different elementary colour-processes, is shewn by the fact that in the case of a white object-light (or one of compound colours such as purple), the series of bright cone-bands are white (or purple). If there were no such accurate synchronisation of these reactions we should find these compound processes analysed to some extent under these conditions[1]. And that this synchronisation is not absolutely perfect is shewn, I think, by the phenomenon well known under the name of Fechner's colours. These are brilliant scintillating colours seen on fixating any point of a disc made up of black and white sectors, and rotating at about 5 rev. per 1″, under bright sunlight. It is usually said that violet predominates, and that is true under certain conditions; but with very bright sunlight and with variation of the proportions of the sectors and of the rate of rotation all colours may be seen, constituting a very splendid and fascinating spectacle. If the synchronisation of the reactions of the retino-cerebral elements subserving different colour-processes is not quite perfect, some such appearance of colours must necessarily result under these conditions.

It is perhaps worth noting in this connexion that these colours appear more readily and are more brilliant in binocular than in monocular vision. Individuals seem to differ considerably as regards the conditions under which they see these colours, and as regards the purity and brilliancy of the colours seen. These individual differences may be due, I would suggest, to differences in the degree of accuracy of the synchronisation of the elementary colour-processes. It may be added that these brilliant colours, resulting from stimulation by pure white light, constitute one of the many phenomena which the theory of opponent colour-processes seems to be incapable of accounting for.

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[1] Charpentier (*Compte Rendu d. Soc. d. Biologie*, 1892) reports that by causing a narrow white image to pass across the field of view while the eye remains at rest and by the observance of special precautions he has been able to analyse the physiological and psychical complex resulting from stimulation of the retina by white light into its constituent colour-processes. I have not hitherto been able to observe this effect.

The influence of the state of adaptation of the eye.

V. Kries[1] has stated that when the eye is completely dark-adapted no 'ghost' can be seen, and that the rod-reaction then follows immediately upon the cone-reaction as a long drawn-out white streamer, so that the circular object-light, which he used, assumes the appearance of a comet. I was at first much puzzled by this description, and could not see how to bring it into line with my general scheme. To arrive at an understanding of the effects of adaptation I have made several series of observations, beginning each with a light-adapted eye, and making observations at short intervals throughout a two hours' sitting in the dark room. A slit 3° wide and transmitting green light of moderate intensity gives a well-marked slightly purplish 'ghost' when the light-adapted eye has been in the dark room two minutes. After five more minutes in the dark room the intensity of the object-light must be a little reduced in order that the 'ghost' may be seen, and it is then grey in colour. Five minutes later the intensity of the object-light must be again lowered a little in order that the 'ghost' may appear, and there then appear between the 'ghost' and the green cone-bands one, two, or more rod-bands (according to the rate of rotation) similar to the 'ghost' but dimmer. From this stage onward it is difficult to obtain a single 'ghost' separated from the leading image by a dark interval, because the whole series of rod-bands tends to be present at all rates of rotation; nevertheless it may be obtained by careful adjustment of the intensity of the object-light and of its speed of movement, even when the eye has become completely dark-adapted. In order to make certain of complete dark-adaptation I have several times put a closely-fitting shade upon one eye before opening it, on awaking after eight hours of sleep in a well-darkened room, and have proceeded at once to the dark room. And I have then been able to obtain by careful adjustment both the complete series of rod-bands, or an interrupted series with terminal 'ghost.' It is necessary to add that these rod-bands, seen by the dark-adapted eye, are, like the rod-bands seen by the partially dark-adapted eye following the cone-bands in case of object-light too dim to give the typical single 'ghost,' extremely delicate phenomena and both practice and patience are needed for the satisfactory observation of them. It is essential too that the object-light should travel upon a perfectly dark ground, a condition which can hardly be fulfilled if, as in the method commonly employed by Bidwell and v. Kries, the object-light is an optical image reflected from a white surface over which it travels.

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[1] *Zeits. für Psychol. u. Physiol. d. Sinnesorgane*, 1896, Bd. XII. S. 91.

But that v. Kries failed to observe any discontinuity in the comet-like white tail which he describes as following the leading image when the eye is completely dark-adapted, was chiefly due, I think, to the fact that he used a circular object-light. It would be impossible to choose a form of object-light less suited to reveal the finer phenomena, because as the circular image travels across the retina the elements over which its centre passes are stimulated for a much longer time than those over which its upper and lower parts pass. Since the intensity and the duration of stimulus are complementary factors (in case of such brief durations as we are here dealing with) the retino-cerebral elements are excited to very different degrees by the central and peripheral parts of the circular image, the reactions of neighbouring elements are therefore not synchronous and the discontinuous or banded character of the total reaction is obscured.

For the accurate observation of the more delicate phenomena it is essential, in fact, that the moving object-light shall be narrow and sharply defined, and that, if it describes a circular path, its borders shall be radii of the circle described. The lack of this precaution has been, I think, one of the chief sources of confusion. Thus Hess has used as object-light a small electric glow-lamp, Bidwell has used a vacuum tube illuminated by electric discharges.

Bidwell has used, too, a circular object-light, and pictures both the leading image and its 'ghost' as well-defined circles. If this were an accurate description of the appearances it would at once throw my scheme of explanation of the phenomena out of court. But as a matter of fact this description is misleading. The leading image appears not as a circle but like the moon when a little less than half-moon (as v. Kries has noted) or like several such half-moons partially overlapping; and the 'ghost' in such a case has a very ill-defined shape.

Summary.

The following are the principal propositions towards the establishment of which I have endeavoured to contribute experimental evidence in this paper:—

1. The primary response of the visual organ to a single momentary stimulus is in all cases (save in the case of a very feeble stimulus) a series of pulses of sensation of diminishing intensity rapidly succeeding one another. The series is longer and the initial pulses are briefer, more intense, and succeed one another the more rapidly, the more intense is the stimulus.

(2) The initial pulses of such a series are predominantly due to processes initiated in the cones of the retina, while the terminal pulses are due to processes initiated in the rods, save in the case of stimulation by pure red light and in the case of the completely bright-adapted eye, in which cases there is no reaction of the rod-apparatus and the series is correspondingly shorter.

(3) The series of pulses of sensation is probably due to a series of discharges of the sensory neurones across synapses of the cortex into the efferent neurones of the sensory-motor path.

(4) The primary response, the series of pulses of sensation is followed after a very brief interval by the secondary response, a steady persistent after-image which is brighter and of longer duration the more intense is the stimulus. This is the ordinary after-image and is due to the continued action in the rods and cones of exciting substances liberated in them by the action of the light upon stored mother-substances (visual-purple in the case of the rods).

(5) The so-called 'recurrent image' or 'Bidwell's ghost' following a moving image and separated from it by a dark interval is merely the last of the series of pulses of sensation, the intermediate members of the series being inhibited by the bright initial reactions which constitute the leading image or images.

(6) The reality of this inhibitory effect may be proved by many different experiments, some of which were described.

(7) The 'ghost' seen in the case of a travelling object-light of moderate intensity is wholly or predominantly due to a reaction of the rod-apparatus, the apparatus for vision in a dim light.

(8) A pure red object-image of high intensity may yield a red 'ghost' very similar in general behaviour to the ordinary 'Bidwell's ghost' save that it does not jump the fovea.

(9) The completely dark-adapted eye may see either a similar 'ghost' or the complete series of rod-responses, when the form of the object-image is suitably chosen and the illumination and speed of movement and other conditions are carefully adjusted.

(10) The sensations of different quality, due to excitation of the cones, are developed at the same rate when they are of about the same intensity; but the more intense sensations are developed slightly more rapidly than the less intense.

(11) The colourless sensations which arise from excitation of the rods are developed more slowly than those due to excitation of the cones, the interval between the moment of stimulation and the moment of the appearance of the sensation in consciousness being greater in the case of the former by about $\frac{1}{18}$ second.

(12) This considerable difference in the rate of the physiological processes underlying the colourless sensations due to the cones and those due to the rods respectively, affords a strong support to the view (advanced by v. Kries) that the colourless sensations having these two origins are determined by physiological processes entirely separate and different in character.

NOTE.

THE PRINCIPLE UNDERLYING FECHNER'S 'PARADOXICAL EXPERIMENT' AND THE PREDOMINANCE OF CONTOURS IN THE STRUGGLE OF THE TWO VISUAL FIELDS.

By W. McDOUGALL.

FECHNER'S paradox consists in this, that if a white area is presented to one eye and then a dull grey area of similar form and size to the corresponding parts of the other eye, the sensation of brightness referred to this common area is not increased by the additional stimulus of the light coming from the grey area, but is diminished by it. The phenomenon remains a paradox, and no satisfactory explanation has yet been suggested. I wish to submit for criticism the suggestion that it, together with the familiar phenomenon of the predominance of contours, is to be explained by a principle, that may be enunciated as follows:—

A visual stimulus of given intensity and extension sets free a certain quantity of nervous energy which, flowing through the conduction-paths of the cortex, is the physical basis of the sensation of light. Each element of the conduction-path corresponding to unit of area of the retina, tends to draw to itself as much as possible of the total energy set free and so to contribute to the whole sensation the most vivid possible sensation-element[1]. This competition for the energy set free is keenest between immediately adjoining elements, and diminishes rapidly with increasing distance. *While the cortical paths of two corresponding elements of the two retinæ are not identical, they are yet so related that either one stands in the same relation, as regards this competition, to the elements surrounding the corresponding element of the other eye as to those surrounding itself.* In the case of the 'paradoxical effect' I base this suggestion on the following facts: (1) if fixation of the combined bright and dark areas is prolonged, some 'struggle' usually occurs, *i.e.*, alternate brightening and darkening; (2) if the darker field is presented before the brighter no 'paradoxical effect' is produced; (3) the effect is greatest when the two areas coincide, if either overlap the other the effect is proportionally lessened; (4) if the darker area is made considerably smaller than the brighter there is no paradoxical effect, but the two brightnesses sum together at once to form a bright area surrounded by a darker zone; (5) the same holds true if the darker field is made up of a number of small areas separated by black spaces. The suggestion is, therefore, that the fatigue of the cortical paths, induced by the preliminary fixation of the brighter field and the consequent increase of the resistance of those paths, enables the paths excited by the darker field to draw off a part of the energy of the excitation-process from the former, diminishing the brightness of the sensation-elements contributed by them.

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[1] The nature of the nervous mechanism, by which this competition is effected, is suggested in the paper on "The Nature of Inhibitory Processes within the Nervous System," *Brain*, 1903, Vol. xxvi. p. 175. The same paper contains a description of the experiments and of the apparatus by the aid of which the facts described in this note were observed.

Now we know that a white surface seen by two eyes appears only very slightly brighter than the same surface seen by one eye, whereas when the illumination of a white surface presented to one eye is doubled the brightness or intensity of the sensation is considerably increased. That is to say, summation of physiological effects of two stimuli is more efficient than psychical summation. Hence in the case of the paradoxical effect, the psychical summation does not compensate for the physiological subtraction from the energy of the excitation-process initiated by the brighter light[1].

The application of the principle to 'predominance of contours' may be illustrated by the following simple cases: (1) To the left eye present a large white disc, and to the corresponding area of the right eye an equally large dull grey disc with a smaller black disc at its centre. The centre of this black disc will then appear grey or white, shading off to the margin, which is black, *i.e.*, the contour predominates; if now, by a simple arrangement, the brightness of the grey area presented to the right eye is gradually increased, the light about the centre of the black disc gradually disappears until the whole disc is equally dark; (2) to the left eye present a white field, to the right eye a completely dark field—the dark field has no influence on the white one; then illuminate gradually and evenly a small disc at the centre of the dark field—this small disc appears at once as a brighter area on the white field, its border being surrounded by a darker zone; as its brightness increases this zone of diminished brightness spreads out over the whole field of the left eye, remaining most marked in the immediate neighbourhood of the brighter disc. In each case the area that is bright to one eye only is subject to the competition of the adjoining areas of both eyes, and the parts of it adjoining the area of summated competition are the first to suffer loss of brightness. Helmholtz and others have regarded the predominance of contours as due to attention being drawn to the contours. I accept this view, but seek to elucidate the physiological factors of the process, and I suggest that competition of nerve-paths for the supply of free nervous energy is the physiological principle underlying the negative or inhibitory aspect of attention. I suggest also, with the greatest diffidence, that the same principle may yield an explanation of the phenomena of reciprocal innervation and inhibition described by Prof. Sherrington.

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[1] Prof. Sherrington's experiments recorded in this Journal seem to shew that there is no psychical summation. If that is the case, my hypothesis is in no way invalidated by the fact, but rather is supported by it.

PROCEEDINGS OF THE PSYCHOLOGICAL SOCIETY.

The following are the titles of the papers which have been read at the Psychological Society since its foundation. In future, it is hoped to give a fuller account of the proceedings.

Feb. 15, 1902.	The Evolution of Laughter, by James Sully. Fechner's Paradoxical Experiment, by W. McDougall. Pathological Changes in Immediate Memory and Association, by W. G. Smith.
May 3, 1902.	Psychophysical Parallelism, by James Ward. A Contribution to the Analysis of the Process of Comparison, by F. N. Hales. Colour Nomenclature, by W. H. R. Rivers.
June 12, 1902.	The Effect of Practice on Certain Simple Mental Activities, by W. G. Smith. A Study in Contrast, by C. Lloyd Morgan. Remarks upon Central Nervous Activity and Efferent Nerve Cells, by Francis Gotch.
Dec. 6, 1902.	The Fluctuations of the Memory Image, by F. N. Hales. A Dream-experience tending to bring certain alleged Characteristics of Consciousness under Anæsthesia into line with those of the ordinary Dream-consciousness, by W. R. B. Gibson. The 'Feeling of Innervation'—a Suggestion, by W. McDougall.
Jan. 31, 1903.	Time Judgment, by Beatrice Edgell.
May 2, 1903.	Demonstration of various types of Insanity, by Robert Jones. The Functions of the Frontal Lobes, by J. S. Bolton. Bilateral Cortical Lesions causing Deafness and Sensory Aphasia, by F. W. Mott.
July 25, 1903.	Comparison of Mental Tests applied to Epileptic Patients, by W. G. Smith. Individual and Racial Differences in Sensory Illusions, by W. H. R. Rivers. The Teaching of Psychology in America, by C. S. Myers.
Oct. 24, 1903.	A Method for tracing the Development of Secondary Emotions, by A. F. Shand. The Taste-names of Primitive Peoples, by C. S. Myers.

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THE TASTE-NAMES OF PRIMITIVE PEOPLES.

BY CHARLES S. MYERS,
Lecturer in Experimental Psychology at King's College, London.

I. *The cautions to be observed in drawing conclusions from primitive vocabularies. Experiments in the Torres Straits.*
II. *Indo-Germanic taste-names.*
III. *The results of inquiry and experiment in other parts of the world.*
IV. *General comparison of the vocabularies denoting gustatory and other sensations. The phylogenetic antiquity and the consequently difficult analysis of the fused product of gustatory, tactual and emotional experience.*

I.

THE study of the words given by primitive people to describe their sensations has hitherto been confined for the most part to a single field, the colour-sense. But the results warrant an extension of such inquiries to other senses. The colour-vocabularies, collected from various regions, show instances where a single word does duty for two or more colour-sensations, *e.g.* for black and blue or for blue and green; where brown receives no definite name; and even an instance where there is apparently but one specific colour-name, a word for red. A similar lack of differentiation exists, as we shall see, in primitive taste-vocabularies[1].

One is naturally prone to conclude that such features of confused nomenclature imply corresponding limitations in the degree of sensory acuity or discriminability. Yet, even if it be certain that dark-skinned people are relatively less sensitive to the violet, and more sensitive to the red end of the spectrum than are more advanced communities, this difference is probably insufficient to account for the surprisingly common absence of a specific word for blue in primitive colour-vocabularies[1]. Moreover, there are unquestionably other factors at work in determining the differentiation of the names of sensations. In the first place, it is quite conceivable that a sensation may not be of sufficient value or interest to receive a special name, although it is capable, nevertheless, of being fully experienced and discriminated. Were we, for example, to ask of a civilized community what objects they would describe by the words grey or bitter, the variety and incongruousness of their replies would be generally surprising. Yet such confusion is clearly not the expression of deficient sensitivity or discriminability; these people could easily learn the exact difference in meaning between the words grey and buff or between the words bitter and acrid. And in the second place, even when a lack of *discrimination* is found, the absence of *discriminability* is not necessarily implied. Certain instances of confused nomenclature may be simply due to insufficient pains having been taken to analyse sensory experiences. The ability to differentiate may exist potentially, and may, if occasion later arise, be carried into actual effect; just as any one of us can be taught by practice to resolve an impure tone into its constituent fundamental and higher partial tones.

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[1] While these pages were in the press, my friend Professor A. F. Chamberlain published an interesting paper on the taste-vocabularies of American Indians (*Amer. Journ. of Psychol.* 1903, vol. XIV. pp. 146—153), which confirms the general conclusions I had previously communicated to him.

But the attempt to explain the imperfections of primitive sense-vocabularies will be seen to involve a discussion of the play of psychological factors in the process of sensation generally; and into this obscure subject we cannot enter now. All that we would do here is to draw attention to a certain, though imperfect, correspondence of the features of taste- and of colour-nomenclature in primitive languages, and to the consequent possibility that their true explanation will be ultimately found to have a psychological rather than a merely physiological basis.

My attention came to be drawn to the taste-vocabularies of primitive peoples in the course of a few experiments which I made during my visit to the islands of the Torres Straits. Dr Seligmann and I tested the islanders with dilute solutions of sugar, salt, acid and quinine, and thereby obtained their equivalent words or expressions for sweet, salt, sour and bitter. The most interesting results of this inquiry can be thus summarized[1]:

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[1] For a full discussion of primitive colour-vision see the following papers by W. H. R. Rivers,—*Reports of the Cambridge Anthropological Expedition to Torres Straits*, vol. II. pt. 1, Cambridge, 1901, pp. 48—96; *Popular Science Monthly*, vol. LIX. 1901, pp. 44—58; *Journ. Anthrop. Inst.* vol. XXXI. 1901, pp. 229—245.

(1) The literal meaning of the phrase commonly used in the Torres Straits to denote sweetness is 'tasting good.'

(2) The same phrase is applicable to denote saltness.

(3) The usual word for saltness is derived from sea-water.

(4) The taste-names for salt and sour tend to be confused.

(5) There is no specific name for the bitter taste.

II.

Now if we examine the taste-names of Indo-Germanic languages, we shall find precisely these features repeated there[2].

(1) The original meaning of the Sanskrit √*svad*, to which the Greek ἡδύς, the Latin *suādeo*, and the English *sweet* are allied, was 'tasting pleasantly.' Apparently only later did the word acquire the narrower gustatory sense of sweetness; primarily it meant 'savoury' or 'tasty.'

(2) Thus it might with equal appropriateness have been applied to salt and to sweet flavours, salt having been almost universally prized by primitive people, and its taste enjoyed by them with relish. Indeed the Lithuanian word *saldùs* (cf. the Latin *sal*) thus acquired the meaning of spiciness and was thence applied finally to sweet flavours. Probably the old Slavonic *sladŭkŭ* and the modern Russian *sládkij*, which likewise mean sweet, had a corresponding origin. The Sanskrit *lavaṇa*, = salt, similarly came to be applied to lustre, grace and beauty; the same usage occurs also in Arabic, Persian and other languages.

(3) Our own taste-word salt, of course, is derived as in the Torres Straits from the tasted substance, the sea; and words with similar derivation prevail throughout Europe. Sweet is probably our only taste-word that had from its very origin a gustatory meaning; indeed the Indo-Germanic *svādus* (= sweet) is possibly related to *su* (= good) and *ed* (= to eat)[3]. Many European taste-names have been either borrowed or evolved from names which owe their origin to our other

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[1] For a fuller account see the *Reports of the Cambridge Anthropological Expedition to Torres Straits*, vol. II. pt. 2, Cambridge, 1903, pp. 186—188.

[2] To facilitate comparison, the figures prefixed to the last paragraph correspond to those in the two subsequent sections.

[3] This conjecture I owe to Mr H. M. Chadwick, of Clare College, Cambridge.

senses. Thus the Latin *acidus* is derived from *aceo*, the Sanskrit *tikta* from *tij* (both of which mean to sharpen), our own *bitter* from the Anglo-Saxon *bítan* = to bite, and *tart* perhaps from the Anglo-Saxon *teran* = to tear[1]. As for the derivation of *dulcis* and γλυκύς and the relation of ὠμός, *amārus*, and the Sanskrit *amla* to each other and to the Semitic equivalents for bitter, they are too uncertain for inclusion here.

(4) The terms salt and sour are often confused in Europe. The peasants of Marchfeld, writes Mach[2], describe salt as *sauer*, "because the word *salzig* is not familiar to them." The Lithuanian word for salt is *súras* (cf. Anglo-Saxon *súr* = sour). Kiesow[3], in the course of his experiments upon children, found that almost without exception sour was called salt, and that a few persistently called bitter salt, until "their judgments were corrected by repeated practice."

(5) I made a few experiments in a small village of Aberdeenshire, and even among the adults I met with precisely the same inability to give a distinctive taste-name to the dilute solution of quinine as I had found in the Torres Straits: they would call it a 'sort of acid' or a 'sort of salt' taste. To some extent, doubtless, this confusion of bitter with sour and salt is attributable to the similar tactile impressions which tend to accompany all three taste-sensations, varying according to the taste and its intensity from mere astringency to acridity, burning, and even pain. But apart from this it is quite certain that most people have a very vague idea of the sensation to which the word bitter should be restricted. In bygone days the word was applied in England to pungent, biting and saline tastes[4], and we still speak loosely of the 'bitter brine' and the 'Bitter Lakes.' In Latin, the word *amārus* was similarly used. In Greek πικρός was applied to sea-water, unripe fruit and to pungent flavours, while in Sanskrit the corresponding *tikta* is found used to describe the taste of a gourd, of mustard and other burning tastes.

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[1] W. W. Skeat, *Principles of English Etymology*, First Series, Oxford, 1892.

[2] *Contributions to the Analysis of the Sensations* (Eng. Trans.), Chicago, 1897, p. 42, footnote.

[3] *Philosophische Studien*, Leipzig, 1894, Bd. x. S. 343, 344.

[4] *The Century Dictionary*, New York, 1889.

III.

The inquiry, thus begun, was extended to other primitive peoples by means of a *questionnaire*, which I addressed to officials, missionaries and Europeans resident abroad. The trouble taken by these unknown friends to secure for me reliable information has in many cases been surprisingly great. Very often they expressly tested the natives with dilute solutions of tasting substances in the manner which I had prescribed. My hearty thanks are due to them for the kind feelings that led them to interrupt the routine of their (often busy) lives in order to furnish me with the material I desired.

It is hardly necessary to point out the errors to which one is liable in an inquiry of this kind. Throughout I have used my discretion in selecting the most trustworthy returns[1]. As a rule, it was easy to gauge from the general character of the written reply how much care or knowledge had been brought to bear in answering the *questionnaire*. Perhaps the best evidence of reliability is furnished by the general broad agreement of the replies from very diverse parts of the world.

The questions which I asked were the following:—

"I. By what word or words in their own language would natives describe the taste of solutions (1) of sugar, (2) of salt, (3) of *weak* acid, (4) of quinine (*i.e.* the tastes we call sweet, salt, sour, bitter)? [The value of the results will be much enhanced if opportunity allow of specially conducted experiments by applying the above solutions to the tongue of even a single native. Specially mention if the given information is based on such experiments.] Possibly the same word is given to two tastes."

"III. Give, if possible, the exact meaning of the words in I. [For instance, are the taste-words borrowed from the tasted substances (as in our English 'salt')? Had they the original meaning of softness, sharpness, etc., or have they given rise to such meanings secondarily? Or do they mean merely pleasant and unpleasant?]"

(Questions II., IV., and V. were not concerned with the subject now at issue.)

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[1] In a few instances, by the advice of Mr S. H. Ray who has kindly read through my manuscript, I have made slight alterations in spelling. I have, I hope, acknowledged all the various sources of my information in the footnotes. But in order to save burdening this paper with excessive detail and unnecessary repetition, I have omitted many otherwise valuable returns, especially those which reached me after the manuscript had gone to press. I am indebted to Professor Bendall and Mr E. C. Quiggin of Gonville and Caius College, and to Mr E. H. Minns of Pembroke College, Cambridge, for help in the languages with which they are specially familiar.

(1) In Uganda[1] there are two taste-words, *kuwoma* and *kawa*, the former being applied to sweet, salt and other agreeable flavours, and the latter to unpleasant flavours. Thus, *kuwoma* is used for the taste of sugar and for that of certain sour fruits liked by the Baganda. In Aulua Malekula, in the New Hebrides[2], a similarly primitive taste-vocabulary is found: the words *garakhar* and *kokon* are respectively used for pleasant and unpleasant tastes.

(2) The use of a common word, applicable both to sweet and salt tastes, has been already alluded to. It occurs also (*a*) among the Golo folk of the central Sudan[3], (*β*) in the Akan dialect of Ashanti[4], (*γ*) in three dialects spoken south of Lake Victoria Nyanza[5], (*δ*) in Dakota[6], (*ε*) in the Bugotu dialect of Isabel Island[7], (*ζ*) in Yoruba[8], and (*η*) in Swahili[1, 9]; the respective words, each of which was used by the natives to describe both sweet and salt tastes, being (*a*) *don-du-teh*, (*β*) *de*, (*γ*) *kunoña*, *kunulila*, *kujomelela*, (*δ*) *skuya*, (*ε*) *mami*, (*ζ*) *dun*, and (*η*) *tamu*.

(3) The taste-word for salt, where it exists, is commonly derived from sea-water. Often a foreign word, having this origin, has been introduced, as in New Britain, the Loyalty Islands, Samoa, etc. Salt is not mentioned in the Rig Veda. In Hindi[10] *khari* (= salt) is derived from *khar*, a dry grass which is burnt to produce the substance. Salt is similarly made in Uganda, but their (Lusoga) word *muka* (= salt taste) has not an obvious derivation. In many tongues, *e.g.* in the Nyoro dialect of the Toro people, East Africa[11], there appears to be no word for the salt taste: they say *okunulira* = it has a pronounced flavour. So, too, the Ainu phrase[12] returned for sweet is *kera an* = there is flavour; and the Malay *manis* probably has a similar meaning, as it is often coupled with the word *banya* (= very), even when only a slight degree of sweetness is implied. Likewise, the Persian word for sweet is *iatli*, derived from *iat* = taste.

But the Persians also use *shirin* for sweet, derived from *shir* = milk. A sickly sweet taste is called *prítornyj* by the Russians, who also employ it to denote insipid and fatty tastes. The Toaripi (British New Guinea) word for sweet likewise means fat. *Kami-kamidha*, the equivalent for sweet in the Bau dialect of Fiji[13], is applied to tender pork and to succulent food generally. Such an association of sweet and fatty tastes is not easy to explain[1]. It may conceivably have arisen from the soft smoothness of very weak or from the syrupy nature of very concentrated sweet solutions; but a truly fatty taste accompanies bitter fluids rather than sweet[2]. Probably our word 'luscious' is a guide to the fitness of these words.

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[1] Rev. J. Roscoe. [2] Rev. T. Watt Leggatt. [3] Captain S. L. Cummins, R.A.M.C.

[4] Anonymous correspondent. [5] Rev. F. H. Wright.

[6] Stephen R. Riggs, *Grammar and Dictionary of the Dakota Language*, Smithsonian Institute, Washington, 1852.

[7] Rev. H. Welchman. [8] Rev. J. Harding. [9] Rev. A. Crabtree.

[10] Captain A. Mudge. [11] Mrs Fisher. [12] Anonymous correspondent.

[13] Dr Glanvill Corney, Mr H. Monckton, and Captain W. W. Wilson.

A further difficulty comes from the north-east of British New Guinea. The Binandele people[3] rendered sweet by *be damo-damo*, which means 'cooling the mouth.' From an independent source[4] I obtained a corresponding word from the Bou people, who live in the mountains near the Wamira district of the same island; their expression for sweet is *wainuau baubana*, which means 'cooling the heart.'

It is natural to find sweetness associated with honey. But there is little evidence to show that honey gives the name to its taste as sea-water so commonly does. On one occasion a Mabuiag islander of the Torres Straits[5] called the sweet solution *wam mitalnga* = thing tasting of honey; but his generic word for sweet is *kapu mitalnga* = thing tasting good. The word returned from the Endeavour River district, Queensland (Koko-Yimidir dialect[6]), is *murla* = honey or bee. The Awalama folk near Wamira, British New Guinea[4], called the sweet solution *wigougauna* = like honey. The Hindi and Punjabi *mitha*[7], = sweet or pleasant to the taste, is allied to the Sanskrit *madhura* (cf. *madhu* = honey, ? *mad* = to delight[8], and our English *mead*).

Our word *sour* may have a twofold connexion. It is probably allied to the sour-tasting sorrel, and by some it has been related to ξυρός (√ ξυ = to scratch). Analogous connexions are commonly met with in other languages. Thus, the Sanskrit *amla* = sour is used for sorrel and is possibly related to *ampfer*, the German word for this plant. So also the Hindi[7] *chuk* (= sour) is used for the French sorrel (*Rumex montanus*), the Punjabi[7] *khata* may be connected with *khat*, a very sour plant, and the Fijian[9] *wiwina* with the fruit *wi*, the Brazilian plum, which is very sour when unripe. Less frequently the word for sour is derived from 'turned' milk. Corresponding to the just-mentioned relation of sour to $\sqrt{\xi\upsilon}$, we have in Malay[1] *gigi nilu* = the teeth stand on end; in the Wamira district of N.E. British New Guinea[2,3] *i vigora* = it bites; in Taupota of this region[2] *i vivo turu-turaa* = it kills the teeth; in Mukaua, also of this region[3], *i i nao bai raumite-mitena* = it causes the face to be wrinkled; in Mota, Banks' I., *gogona* from *gona* = tied up in a knot[4]. The Nyoro[5] word for sour is *enturo* = spittle-creating, and the Malagasy word[6] is *mahari-kivy*, with precisely the same meaning.

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[1] It is a curious coincidence that Aristotle (*De Anima*, lib. II. cap. 10 § 5) expressly places the fatty or oily taste near to the sweet.

[2] F. Kiesow, *op. cit.* S. 525. Oehrwall, *Skandin. Arch. für Physiol.* 1894, Bd. II. S. 9.

[3] Rev. Copland King.

[4] Rev. E. L. Giblin.

[5] Dr C. G. Seligmann.

[6] Rev. G. H. Schwarz and Dr W. E. Roth.

[7] Captain A. Mudge.

[8] Cf. A. Fick, *Indogermanisches Wörterbuch*, Göttingen, 1890, Th. I. S. 105.

[9] Dr Glanvill Corney, Mr H. Monckton, and Captain W. W. Wilson.

(4) Confusion between salt and sour, and more especially between salt and bitter taste-names, is frequently met with. In New Guinea[3], in the New Hebrides[7], and throughout a great part of Polynesia, a common word is used to denote salt, sour, and bitter tastes. The Arabic *ḥamid* is applied to salt and bitter as well as to sour tastes. The Somali *danan* is applied to lemons and to salted meat. All these words doubtless mean rough, unpleasant or biting.

(5) In Aulua Malekula, New Hebrides[8], in the Suto language of the North Transvaal, in Uganda[9], in the Bugotu dialect of Isabel Isl.[10], and in Toaripi, British New Guinea[11], the respective words *kokon, ba-ba, kawa, aha* and *eakere* equally express either sourness or bitterness. A notion of distaste, astringency or pain underlies all these words. The Russian *gor'kij* = bitter, with which perhaps *gorět'* = to burn is connected, properly means pungent; so also the Sanskrit *kaṭuka*, which is probably connected with √*kriṭ* = to cut (cf. Lithuanian *kartùs* = bitter). The Mukaua folk[3] called the bitter solution *i i kawasiqariqari* = it irritates the mouth; those of Bou[2], also in British New Guinea, called it *waipolana* = burning; those of Awalama[3], of the same district, *witururuana* = causing numbness[4]. The taste-name for bitter among the Suto folk of the N. Transvaal[9] is *ba-ba* = astringent; among the Kaguru of Central Africa[10], it is *usungu* = painful; among the Gogo folk[11]

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[1] Mr H. N. Ridley. [2] Rev. Copland King. [3] Rev. E. L. Giblin.

[4] It is naturally impossible to say how far these and other returns represent the generic taste-words in ordinary use and how far they were invented on the spur of the moment by the subject under investigation. Several of them, however, were corroborated independently by other natives, although others may have been specially employed for the occasion, as Dr Rivers found to be sometimes the case in the colour-nomenclature of Torres Straits (*op. cit.* p. 61).

[5] Mrs Fisher.

[6] Rev. J. Richardson, *A New Malagasy-English Dictionary*, Antananarivo, 1885.

[7] Rev. W. Watt. [8] Rev. T. Watt Leggatt.

[9] Mr P. C. Jonas. [10] Rev. A. North Wood. [11] Rev. H. Cole.

kali = fierce; and in Fiji[1] *mbaku* = astringent, or *nga-nga* = pungent. In several languages it is also used to mean 'poisonous.'

IV.

The features with which we thus meet in taste-vocabularies are to some extent represented in vocabularies pertaining to other senses. Just as sour and bitter or sweet and salt are often described by a common name, so the words for blue and black are usually found undifferentiated in primitive languages. The derivation of the word for saltness (and sometimes of that for sourness) from the tasting substances is paralleled by the fact that most colour-names have been borrowed from the names of the coloured objects. Again, as in taste, so in hearing recourse was had to words expressing tactual or muscular sensations, when tones were classed according to their pitch, as *acutum* or *grave*, or as *ὀξύ* or *βαρύ*.

To primitive man, of course, brown and grey were presentations as unique as blue or red, differences of *timbre* seemed as elementary as differences of pitch, and astringency, oiliness and alkalinity were gustatory sensations as simple and pure as sweetness and bitterness[2]. But in respect of later analysis taste has always laboured under special disadvantages, compared with the other four senses. The frequent association of sensations of taste with those of olfactory origin and their intimate fusion with disturbances of tactual or common sensibility have always made investigations very difficult. In few senses as in taste do we find such feeble powers of subjective analysis, or such a close relation with general emotional tone. Long ago it was written of tastes,—*τοῖς πάθεσι καὶ ταῖς ἡδοναῖς διαφέρουσι*[1]. And the most primitive taste-vocabularies, as we have seen, are based upon mere changes in affective tone. Substances are primarily classed according as they are tasteful or distasteful; the differences, for example, between sour and bitter are considered less striking than their common impalatability.

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[1] Dr Glanvill Corney, Mr H. Monckton, and Captain W. W. Wilson.

[2] The following taste-vocabularies are of interest in this connexion:—

(*a*) *Sanskrit*: sweet, salt, sour, bitter, pungent and astringent. Six tastes were so universally recognized in ancient India that the tastes became the chronogram for the number six. In the Suśruta the *gustus aquarius* is described as being composed of six tastes, of which sweet, salt and acid are pleasing and heavy, bitter, pungent and astringent are rough and light.

(*β*) *Newārī*, a Tibeto-Burman language of Nepal, from the 14th century onwards: sweet, salt, sour, bitter, astringent and savoury. (Cf. Conrady, *Ztsch. d. deutsch. morgenländ. Gesell.* Leipzig, 1898, Bd. XLVII. S. 558.)

(*γ*) *Greek*: sweet, salt, sour, bitter, astringent, dry, pungent, vinous and fatty (or oily). (Aristotle, *De Anima*, lib. II. cap. 10 § 5; Theophrasti Eresii *Fragmenta, De sensu et sensibilibus*; Plutarch, *Questiones naturales*, v.)

(*δ*) *Mediæval Italian*: sweet, salt, sour, bitter, astringent, dry, fatty, insipid and acrid. (Joanne Bravo, *De saporum et odorum differentiis*, Venetiæ, 1592.)

(*ε*) By *Luchtmans*, 18th century: sweet, salt, sour, bitter, astringent, vinous, alkaline, unctuous, insipid and acrid.

(*ζ*) By *Linnaeus*: sweet, salt, sour, bitter, dry, aqueous, mucous, fatty, styptic and acrid. (The last three classifications are cited from W. Horn, *Ueber d. Geschmacksinn d. Menschen*, Heidelberg, 1825.)

It seems likely that the intimate connexions between sensations of taste, touch and emotional tone, to which the vocabularies of primitive peoples thus bear witness, date back to a very early period of phylogenesis. For in the skin of fish occur beaker-shaped organs, which have the same structure as that of mammalian taste-buds and are continuous with similar end-organs within the mouth. And in children it has been shown that gustatory sensibility is spread throughout the mouth and pharynx far more widely than in adult life[2].

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[1] Theophrastus, *loc. cit.*

[2] Kiesow, *loc. cit.* S. 845—848.

IMMEDIATE MEMORY IN SCHOOL CHILDREN.

By W. H. WINCH,
Inspector of Schools.

I. *The problem stated.* II. *The method of experiment employed and its validity.* III. *Preliminary experiments made in* 1901. IV. *Experiments made in* 1902 *with statistical tables.* V. *Suggested conclusions.*

I. Professor James[1] writes: "One's native retentiveness is unchangeable. It will now appear clear that all improvement of the memory lies in the line of elaborating the associates of each of the several things to be remembered. No amount of culture would seem capable of modifying a man's general retentiveness. This is a physiological quality, given once for all with his organization, and which he can never hope to change."

The importance of this question both psychologically and pedagogically need hardly be emphasized.

Far-reaching pedagogical inferences are being made. To take one instance, Mr Graham Wallas, Chairman of the School Management Committee of the London School Board, said, in an address to the Froebel Society, July 1901, "Now that we have abandoned the hope that the actual sensitiveness and retentiveness of the memory can be much increased by education, etc......."

It seemed to me that experiments on school children might help to throw some light on the psychological question involved, and accordingly I have endeavoured to determine:—

1. Whether pure memory, that is, memory of percepts associated only in time and space, is improvable by practice.
2. Whether such memory tends to improve with age, or not.
3. Whether such memory has any relation to general intellectual proficiency, and, if so, what relation.

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[1] *Principles of Psychology*, vol. I. p. 663. The differences of type in the original have not been reproduced.

II. The method employed was to expose to view sets of 12 consonants for a period of twenty-five seconds, after which they were reproduced in writing by the children.

Some preliminary questions arise as to the validity of this method.

(i) Are associative factors other than those of time and place really excluded by such a method?

I do not wish to prejudge this question so far as adults are concerned, but I think that such associative factors did not influence the children. In the first place the time of exposure was extremely short.

Moreover, with a view of definitely testing this particular point, I gave a series of similar exercises to another group of children, and was unable, even on prolonged questioning, to discover any such factor. Three girls out of forty thought of words suggested by some of the consonants. These girls, however, also asserted that the words came to mind whilst I was questioning, and that they had not thought of them when learning the consonants. One girl remembered that she had thought of h-y-m-n whilst learning the third line of one of the tests, though only three of these letters were given and then in a different order. It is, perhaps, significant that, on turning to her paper, I found this line omitted, though the two preceding lines were correctly given.

(ii) Are we not testing immediate reproductive power only and not final retentiveness? This is a distinction of degree rather than of kind, for one is reproduction after short periods, and the other after longer periods. There may be no direct relation between retentiveness for short and for long periods; the popular notion 'easy come, easy go' may be correct. If, however, we are entitled to assume that success in school examinations implies retentiveness for long periods, and, if it can be shown that in most cases the children who are most successful with the tests given do best in these examinations, some indirect evidence is afforded that retentiveness for long and short periods vary together.

III. A first series of experiments was carried out in June 1901.

A set of consonants, of which the following is an example,

s	*k*	*m*	*w*
b	*f*	*l*	*h*
g	*t*	*r*	*n*

was suddenly exposed to view. Audible repetition was not permitted. At the end of the twenty-five seconds the card of consonants was removed and the children wrote down as much as they could remember.

Two minutes elapsed and a second test was given, then a third. After the third test a change was made; and for the next three tests an interval of twenty-five seconds was enforced after the card had been removed before the consonants were written down. The papers were then collected and marked on the following scale:

1. Three marks for each letter in its right position. 2. Two marks for each letter one remove to the right, or left, or above, or below. 3. One mark for each letter two removes to the right, or left, or above, or below.

Specimen of test given

m	*t*	*d*	*x*
v	*l*	*y*	*m*
s	*z*	*b*	*r*

Specimen of a marked paper

3	3	0	1
m	*t*	*l*	*r*
2	2	3	3
l	*v*	*y*	*m*
2	2	1	1
z	*b*	*s*	*x*

23 marks out of a possible 36.

One set of experiments was made with Standard VII, numbering 25, the highest class of a girls' school. The ages of the pupils ranged from 12 years 1 month to 15 years 11 months.

Another set was made with Standards VI and VII, numbering 33, the highest class of a boys' school. The ages of these boys ranged from 11 years 9 months to 14 years 2 months.

The following is a brief summary of the results.

1. No clear difference in the results appeared between the method which (*a*) allowed no time to elapse before writing down the consonants after the observation, and that which (*b*) allowed 25 seconds to elapse before writing them down after the observation.

All subsequent experiments, therefore, were made without this interval.

2. In the girls' school, where the pupils were all of the same standard, there was a considerable improvement in memory as the age increased from 13 to 14, and a slight fall from 14 to 15.

3. In the boys' school, where there were two standards in one class, the boys of 12 years of age were rather superior to the boys of 13. But out of the 19 children who were 13 years of age five only were in Standard VII, the remaining 14 being in Standard VI. Separating the standards it was found that the average mark for the boys in Standard VII, who were 13 years old, was 24, for those in Standard VI, who were

TABLE I.

Showing (a) *Improvement of individual pupils, and* (b) *A comparison between memory marks, marks for specified examinations, and position in class for all subjects.*

Name	Standard	Average Memory mark				Average for examinations	Position in Class
		1st 10 tests, June 5	2nd 10 tests, June 12	3rd 10 tests, July 3	Average, 3 days		
M. P.	Ex-vii.	25·5	30·3	32·7	29·5	29·5	4th of 26
A. H.	,,	15·3	18·9	26·7	20·3	23·2	19th ,, ,,
A. C.	,,	25·6	32·6	33·6	30·6	26	12th ,, ,,
M. M.	,,	25·6	33·2	31·7	30·1	26·5	16th ,, ,,
M. L.	,,	27·1	30·2	34	30·4	27·2	8th ,, ,,
K. R.	vii.	27·3	30·7	32·7	30·2	22·5	15th of 37
A. J.	,,	22·3	23·5	—	22·9	25	18th ,, ,,
F. H.	,,	21·8	22·8	29·4	24·6	25	20th ,, ,,
W. K.	,,	27	30·8	31·3	29·7	22	30th ,, ,,
A. L.	,,	28·6	31·8	—	30·2	24·7	10th ,, ,,
D. O.	vi.	19·8	27·7	32·6	26·7	26·5	10th of 30
E. E.	,,	31	34	35·3	33·4	26·2	10th ,, 35
W. G.	,,	29	32·8	35·8	32·5	32·5	2nd ,, ,,
E. S.	,,	29	30·9	34·7	31·5	25·2	20th ,, 30
H. W.	,,	25·3	34·9	—	30·1	30	5th ,, 35
A. R.	v.	13·5	22·2	23·9	19·8	27	17th of 50
H. D.	,,	18·3	26·8	34·1	26·4	30·2	5th ,, ,,
V. A.	,,	18·3	20	24·9	21	24	30th ,, ,,
E. L.	,,	23·3	26·3	27·9	25·8	25·7	25th ,, ,,
H. H.	,,	18·8	19·4	20·9	19·7	22·7	27th ,, ,,
F. D.	iv.	21·3	23·2	26·1	23·5	31·2	8th of 59
D. Y.	,,	23·8	26·3	27·6	25·9	32	14th ,, ,,
E. A.	,,	23	26·2	27 2	25·4	33·7	6th ,, ,,
W. W. ...	,,	16·8	21·7	22 5	20·3	30 7	12th ,, ,,
M. O.	,,	18·3	25·7	29·6	24·5	35·7	2nd ,, ,,
F. P.	,,	25	26	—	25·5	28·2	22nd ,, ,,
H. W.	iii.	12 6	17·4	18·1	16	30·2	18th of 66
D. J.	,,	15·6	17·8	15 5	16·3	30	20th ,, ,,
D. H.	,,	10	15·4	19·2	14·8	32	8th ,, ,,
M. M.	,,	11·8	17·7	24	17·8	28 2	9th ,, ,,
M. H.	,,	19	18·4	22·3	19·9	28·2	4th ,, ,,
F. P.	,,	15·6	19·6	19 1	18·1	27	54th ,, ,,
D. W.	ii.	16·6	18·7	—	17·6	27·7	18th of 62
E. H.	,,	12·1	13·7	—	12 9	14·6	23rd ,, ,,
D. O.	,,	9·1	—	15	12	—	—
N. S.	,,	13 6	17·2	18·2	16·3	22·7	15th ,, ,,
E. M.	,,	16	17·6	15	16·2	19·7	26th ,, ,,
M. G.	,,	11·8	17	20 9	16·5	15·2	60th ,, ,,

seventh column of Table I. As, however, it might fairly be objected that the subjects above enumerated were more especially such as required 'good memory,' it was thought advisable, in addition, to take the actual position of each pupil in her class, taking into account the year's examinations in all subjects, oral and written, and allowing also for the estimate of the teacher in charge of the class. The results thus obtained are shown in the eighth column of Table I.

Conclusions from Table I.

1. That there is a marked and almost invariable improvement in individual pupils.

2. That when the memory marks of the selected girls and their position in class are compared, we find,

(*a*) That general mental ability is accompanied by 'good memory.'

(*b*) That, with two exceptions, no girl whose memory mark is relatively low has a high place in class.

(*c*) That 'good memory,' though usually accompanied by general proficiency, is not invariably so, as there are four cases in which high memory marks are obtained by girls whose positions in class are low.

The general relation, however, between memory, age, standard and position in class, is more clearly indicated in the following summary.

TABLE II.

Showing improvement with age and standard, and the average position of the selected girls in their classes.

Standard	No. tested	Average age	Average Memory mark				Average position in Class
			1st 10 tests	2nd 10 tests	3rd 10 tests	Average, 3 sets	
Ex-vii.	5	14 yrs. 8 mths.	23·8	29·0	31·7	28·1	41st of 100
vii.	5	13 ,, 5 ,,	26·3	27·9	31·1	28·4	50th ,, ,,
vi.	5	12 ,, 8 ,,	26·8	32·0	34·6	31·1	27th ,, ,,
v.	5	11 ,, 4 ,,	18·4	22·9	26·3	22·5	41st ,, ,,
iv.	6	10 ,, 5 ,,	21·8	24·8	26·6	24·2	18th ,, ,,
iii.	6	9 ,, 0 ,,	14·1	17·7	19·7	17·1	28th ,, ,,
ii.	6	8 ,, 2 ,,	13·2	16·8	17·2	15·7	45th ,, ,,

These figures show clearly a general improvement rising with age and standard. It is significant, perhaps, in this connexion, that the common opinion among teachers, to be found also in text-books of pedagogy, is that this 'rote' memory, which we are here testing, is strongest at about 10 years of age, and then diminishes.

It will be seen that, in those cases where the memory mark of a younger section is higher than that of an older section, the pupils in the former are considerably above average intellectual proficiency, as shown by their position in their classes.

The very high mark in Standard IV is explained by the position in class of the selected girls. They are an exceptionally capable group.

The Standard VI group is again of unusual capacity.

In order to bring the relationship between 'rote' memory and general proficiency to a more conclusive test, a fourth series of exercises was given. Ten sets of consonants were used, the time of exposure was 25 seconds, and the intervals between the tests were as before. Girls were selected from two divisions of Standard VI, six from the upper, and six from the lower. None of them had been submitted to any of these tests before. The summarized results are given in Table III.

Table III.

Name	Age	Average Memory mark	Position in Class
Upper Division:			
L. W....	12 yrs. 11 mths.	30·6	1st of 35
M. G.	12 ,, 9 ,,	29·3	3rd ,, ,,
M. S.	12 ,, 11 ,,	26·1	6th ,, ,,
E. S.	13 ,, 3 ,,	24·8	8th ,, ,,
F. M.	12 ,, 0 ,,	29·1	9th ,, ,,
G. B.	14 ,, 4 ,,	21·9	11th ,, ,,
	Av. 13 yrs. 0 mths.	Av. 26·9	
Lower Division:			
E. H.	14 yrs. 0 mths.	19·9	25th of 30
E. W.	12 ,, 6 ,,	18·3	26th ,, ,,
M. G.	14 ,, 0 ,,	20·6	27th ,, ,,
M. P.	13 ,, 3 ,,	14·1	28th ,, ,,
D. R.	13 ,, 0 ,,	26·2	29th ,, ,,
O. R.	12 ,, 6 ,,	15	30th ,, ,,
	Av. 13 yrs. 2 mths.	Av. 19	

The summary seems conclusive as to the relations, above suggested, between general proficiency and good memory. The only girl with a low memory mark in the upper section is much older than the others in that section, and in the lower division we have a case (D. R.) in which good memory is not accompanied by high position in class.

I lay stress on the suggestion that general intellectual proficiency is usually accompanied by 'good memory' (using the term in the limited sense given in these experiments), since Professor Ebbinghaus[1], using an auditory test, finds that the children in the lower part of the classes experimented upon were superior in memory to children who were higher in class.

A different relation between visual memory and general proficiency may exist from that which obtains between auditory memory and general proficiency. In German schools, however, the oral teaching is greatly in excess of that in English schools; and I have myself been profoundly impressed by the ability of the German school child to answer questions and perform calculations which were presented to him orally.

It would be inferred from this, if practice has the same effect on auditory memory as it appears to have on visual memory, that the more instructed children should show superiority. This inference is, however, contradicted by the results of Professor Ebbinghaus' experiments.

In any case, apart from the value, if any, of the conclusions arrived at, my experiments yield a body of facts concerning immediate memory in English school children obtained by a method which renders comparison easy.

V. Recapitulation of conclusions suggested by these experiments.

1. That 'pure memory' is markedly improvable by practice.

2. That this memory improves with age within the limits chosen, but principally in so far as increased age itself implies increase of general proficiency.

3. That there is generally a direct relation between 'good memory' of this kind and intellectual proficiency, so far as this can be measured by success in school studies.

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[1] "Ueber eine neue Methode zur Prüfung geistiger Fähigkeiten und ihre Anwendung bei Schulkindern," *Zeits. f. Psychol. u. Physiol. d. Sinnesorgane*, 1897, Bd. XIII. S. 401.

when she was two years old. The rest—a brother and three sisters—are said to have been able to see perfectly well. The sister who was born blind, now 33 years of age, was brought up in the Blind Asylum, but the patient himself was allowed to run about as he pleased, no attempt to educate him having ever been made." This sister has also been successfully operated upon by Dr Ramsay, and I have had an opportunity of seeing her. Her case is less interesting than that of her brother, and I shall afterwards say something about the difference between them. To return to John Carruth. He was "so familiar with the district in which he resided" (Bridge of Weir, a small country town in the neighbourhood of Glasgow) "that he could go about without the slightest fear, and he says that one of his greatest pleasures in life was to rise early on a fine spring morning and walk in the country to listen to the singing of the birds, to enjoy the freshness, and to 'have a good think.' His hearing was so acute that he knew at once if there was anything unusual on the road along which he was walking, and thus he never had any difficulty in keeping himself out of danger. The 'sense of obstacles' spoken of by psychologists was indeed developed to such a degree that he hardly ever came in contact with what might be in the way; he seemed to perceive the obstruction as he approached and was thereby enabled to avoid it." On questioning him, however, I could not find that he had any definite experience of the "facial perception" described by the blind author, Mr W. Hanks Levy (*v.* James, *Principles of Psychology*, Vol. II. p. 204). He could not explain his feeling of the presence of untouched objects; but he did not believe it was due to differences in the currents of air striking the face, and he was inclined to attribute it entirely to hearing. "As he passed along a road he could tell a wall from a hedge by the sound of the air coming through the leaves and branches of the latter. He could easily go on an errand to any house in his native village, for the resonance of his footfall—quite different in sound when he was passing a building, from what it was when he was opposite an open space—enabled him, perfectly familiar as he was with his surroundings, to count the houses as he passed, and thus to turn corners and finally stop at the one he wanted. In a strange place, however, he could never trust himself to go about without a guide, because his sense of hearing conveyed nothing to him beyond the difference between passing buildings or open spaces, and number could not come in to render the auditory impressions definite." It should be remarked that, in his native village, the great majority of the houses are detached. In passing along a street he could ascertain his position when there were open entries ('pends') or crossings. He had so trained his sense of hearing that he once, on the road, told a farmer that his horse had lost a shoe, although the farmer was quite unaware of it. He worked for some time as a gardener, and "he learned to pluck flowers, arrange them in bunches and pack them in boxes for market, not only without the slightest difficulty, but with very great accuracy. He distinguished different blossoms partly by touch, but chiefly by smell; and by dint of asking questions, he got at last to know so much about their form and colour, that he could arrange them in a bouquet. He recognised the presence of strangers in the house chiefly by the sense of hearing—for example, he could discriminate persons whom he knew by the sound of their respiration, and he was at once cognisant of any breathing with which he was unfamiliar. Besides this, however, he says that if he came into the house when any strange person was there he experienced a sense of 'fulness.' He is unable to put this in clearer terms, and the feeling may correspond to that ascribed by Wardrop, in his 'History of James Mitchell, a boy born blind and deaf,' published in 1813, to a highly developed sense of smell." In the record of James Mitchell's case, however, there does not seem sufficient ground for connecting this perception of the presence of strangers with the sense of smell. In the account of James Mitchell given by "a clergyman in the county of Moray" (*Collected Works of Dugald Stewart*, Vol. IV. p. 314), it is said that "when a stranger arrives his smell immediately and invariably informs him of the circumstance, and directs him to the place where the stranger is, whom he proceeds to survey by the sense of touch.... It has occasionally happened that visitors have arrived in a carriage, and on such occasions he has never failed to go to the place where the carriage stood." But as James Mitchell was deaf as well as blind, this is merely an inference from the fact that he did not obtain the information by sight (even although his eye had been imperfectly operated on). And Dr Gordon said of him: "His sense of smell is unquestionably extremely acute. But I have not been able to learn any fact which could lead me to believe that he could, in a room at least, discover a person by this sense alone, at the distance of twelve feet....As to a power of determining the *direction* of an object, by some *distinct quality* in its odour, like that quality in sound by which we discover the direction of a sounding body, I could not perceive that he enjoyed any such power more than other persons" (*loc. cit.* p. 352). Carruth's word "fulness" suggests to me that the sensation of the presence of a stranger may possibly be a development of the general (somewhat uncomfortable) feeling of proximity to another person, the feeling of being crowded, even when there is no actual contact. In the case of people who see, the feeling arises only when there is very close proximity; but this may be merely due to the fact that we *see* that the other person is not uncomfortably near. The blind man has no such means of retarding the rise of the feeling. Yet this does not explain why the blind man should observe the presence of *strangers* alone in this way. Possibly he is aware of the presence of his friends in other ways, and has merely a feeling that the small room he enters has more people in it than usual.

To return, however, to the narrative, Carruth "occasionally worked in the harvest-field, and could bind the corn and arrange the stooks as well as any of the other labourers. He says he was even able to build the sheaves on a cart, and naïvely adds that although the load might not look 'elegant,' yet it always remained firm on the cart. At other times he assisted in trimming turnips with a large, sharp knife, and only on one occasion did he cut himself. He was also employed to feed cattle and was guided to their stalls by hearing. As a boy he was a leader in every game and would climb the highest trees, birdnesting as fearlessly as any of his companions." "He was a grocer's assistant for a time, and before he was many weeks in the shop, he knew the weights so accurately that he was able to weigh all forms of dry goods and parcel them up. He also went round the houses every morning for orders, and when he returned to the shop he had instructions from six or eight different people to send goods comprising from forty to sixty different articles. Every one of these he remembered after it had once been told him, and he afterwards packed his basket and distributed the groceries to the customers." This information, I may say, was given in a letter written by the grocer whom Carruth assisted.

II. *Condition of the eyes, before and after the operation.*

"The eyes were small, deeply sunk, and moved continuously in the sockets, and there was a very pronounced alternating convergent squint. The irides were natural, the pupils active, and the intra-ocular tension normal, but both lenses were completely cataractous. The patient was quite unable to distinguish objects, although he could tell day from night, and could easily perceive a light and locate it accurately." He does not, however, "appear to have had any pleasure in its brightness," and he "seems to have had no perception of bright colours." By the operations both lenses were removed, after a "preliminary iridectomy, in order to test the vulnerability of the ocular tissues." "Chloroform was administered, as the patient was quite unable to control the movements of his eyes," and after the operations the bandage had to be removed and dark spectacles substituted, owing to the irritation caused by the constant motion. Writing on the 20th of May Dr Ramsay says: "The squint and ocular restlessness are less pronounced than they were, but the patient has still very little control over the movements of the eyes. When he is requested to look in any particular direction he is unable to cause the ocular muscles to do what he wishes, and the balls oscillate, and one or other turns inwards to such an extent that a portion of the cornea is hidden by the inner canthus. This want of control renders it very difficult to make a satisfactory ophthalmoscopic examination, but as far as can be made out the fundus oculi is normal; indeed, the functional activity of the optic nerves since the cataracts were removed is very remarkable, and is in striking contrast to the purposeless muscular movements." When I saw him on October 23rd both the squint and the ocular restlessness had entirely disappeared, and the eyes seemed to be completely under control.

III. *Psychological observations after the operation.*

(*a*) *Perception.* "For about ten days after the operation on the left eye the patient appeared quite dazed, and could not realise that he was seeing. The size of everything in the ward seemed very much exaggerated, and on that account he had great difficulty in interpreting what he saw, but, as he is inquisitive, and has a keen desire for knowledge, he took from the outset a most intelligent interest in his own case, and asked numerous questions of his fellow-patients." This conversation with his fellow-patients, of course, spoiled him to some extent as a psychological subject. At any rate it must be borne in mind in drawing conclusions from his statements. "The first thing he actually perceived was the face of the house-surgeon. He says that at first he did not know what it was he saw, but that when Dr Stewart" [who was bending over him] "asked him to look down, the sense of hearing guided his eye straight to the point whence the sound came, and then, recalling what he knew from having felt his own face, he realised that this must be a mouth, and that he must be looking at a face." It should be added that he felt his own face with his hand while he looked. "Once he properly understood what vision meant, he made very rapid progress, and his extraordinarily retentive memory enabled him to take full advantage of everything he was told." In this respect he differed from some of the patients in former cases, who learned things only after frequent repetitions. This quickness of apprehension is probably due, in part at least, to his comparatively highly-developed system of knowledge obtained through other senses than sight. He told me, *e.g.*, that he recognised certain objects as pictures, because he knew that pictures hung on walls.

"He was quite ignorant of colour, but learned to distinguish hues very quickly. The first tint that he saw was red. A red blanket lay across the foot of his bed. He asked what it was, and was told; and never afterwards did he have the slightest hesitation in discriminating red again. He was shown a narcissus, and on being asked to describe it he immediately recognised the flower, and knew, from his old bouquet-making experience, that it was white and yellow; but he now for the first time also became aware of the little red band in the centre, and at once called attention to it. When he was shown a bunch of daffodils he recognised them by their smell, and immediately said they must be yellow. The colour that took him longest to master was green, but he can now name all ordinary tints readily and correctly. His difficulty with green is hard to explain, unless it be that with green he has no smell-association such as he had with coloured flowers. That there was such association seems evident from his answers when he was asked to describe a dark ruddy brown, namely, that he did not know what to call it, but he thought it must be like wallflower." The colours with which he was tested were not pure primary colours, and his difficulty with green may have been due in part to the differences in the greens which were put before him, and their approximations to yellow or to blue. His description of a dark ruddy brown as "like wallflower" is very difficult to understand, for he had not *seen* wallflower nor, apparently, had he known beforehand that the colour he was asked to describe was brown. He may possibly have seen other shades of brown and, associating brown with wallflower from his previous knowledge, may have meant that this, which he saw, was a kind of brown. I cannot explain it in any other way.

When I saw him on October 23rd I again tested his power of recognising colours and found that it was considerably reduced in artificial light (electric). On the walls of the room there were large coloured diagrams of the eye—almost entirely red. He took a considerable time (at least half a minute) to identify the colour. There was also a bunch of chrysanthemums (of various colours) in a flower-glass. He passed over some white blooms, which were nearest to him, and looked at some others of a mauve colour. After some hesitation (saying, "I don't like to make a mistake"), he said that they seemed to be red. Then he returned to the white flowers, and recognised their colour at once. I had not remarked on the artificial light, but he drew attention to it himself, and said that he always found it difficult to recognise colours except in sunlight. "Even yellow," he said, "looks different in artificial light[1]."

In connexion with his perception of form, as distinct from colour, some interesting observations were made. "When asked to distinguish between a ball and a toy brick, he looked at them attentively for a considerable time, his hands meanwhile moving nervously, as if he were trying to translate what he saw by comparing it with an imaginary tactile impression, and then he described both correctly" [*i.e.* the one as round, the other as 'square']. "He explained that he was so much in the habit of handling objects that he had come to have a 'notion in his mind' regarding the form of things." This might be taken as, to a great extent, a confirmation of the view of Dr T. K. Abbott (*Sight and Touch*, pp. 141, 2). "The problem," he says, "as usually stated, is not to identify the cube seen with the cube felt, absolutely; but the man is supposed to know that one of the two bodies before him is a cube and the other a sphere, and he is only required to find a visual difference of the same kind as the known tactual difference between them. Now suppose he has acquired by touch the notion of a straight line, this notion is suggested by all the edges of a cube, and is wholly excluded by the sphere. But it cannot be questioned that the idea of a straight line can be obtained by sight, and that in whatever way the cube is looked at, it is bounded by straight lines, which the sphere does not present in any aspect. This furnishes a characteristic quite sufficient for the identification. But sight gives also, I will not say the idea of an angle, but the contrast of continuity and interruption. When the eye traverses an edge of the cube it comes to an abrupt stop, and the look must turn in a different direction several times in the circuit, and a similar change occurs when the cube is handled. But the globe gives to both senses the idea of a continuous uniform outline. Hence we conclude, without reference to the perception of solidity at all, that if the blind are capable of acquiring the ideas in question, they will, on being made to see, be competent to name correctly the globe and cube which they have previously felt. Consequently if, upon being fairly examined, they appear incapable of doing so, it will follow that the defect is not in sight, but in touch, not in their new sense, but in their old ideas." Carruth's 'notion' seems explicable by the first part of Dr Abbott's statement. I found that, before the cube and sphere experiment was made, he had had visual experience of the difference between things straight and curved, *e.g.* the straight backs of chairs, the curves of faces, etc. His 'notion' may have been formed in this way. His own statement about his habit of handling objects and the nervous movement of his hands while he was coming to a decision seem to indicate something like what is described by Dr Abbott in the second part of his explanation. But I do not feel sure about this, for the continual restlessness and absence of control in his eyes seem inconsistent with a recognition of the contrast between continuity and interruption in the movement of the eyes from point to point in the object.

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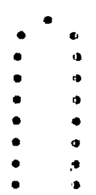
[1] Carruth has since been found to have considerable weakness of vision for red and green, if not actual red-green blindness.

The recognition of form in Carruth's case advanced rapidly. On May 4th I was seated beside him on a bench perforated with holes, forming pointed arches, the lower parts being straight and the upper parts curved. He did not at first recognise the holes as holes but took them for black spots. When I asked him whether they formed straight lines or curves, he answered quite correctly. I then asked if he knew what an arch was. He said he supposed I meant the sort of thing that was sometimes to be found on the top of a garden gate, indicating with his hands a low, flat arch. I asked if the top part of the figure on the bench was an arch. He said "No"; but after looking at it for a few moments he remarked that if the topmost hole were brought down between the two next to it it would be an arch. This seems to me very difficult to explain and at the same time highly suggestive. Before his talk with me Carruth had either not seen anything shaped like a flat arch or, if he had seen such a thing, he had at least not definitely thought of it as an arch. Further, he did not at that time visualise[1], he had no definite visual images, and consequently it was impossible for him to make his statement on the basis of a mental comparison between what he perceived and the visual image of a flat arch. Yet the action of his



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[1] Cf. below, p. 146.

hands indicating what he supposed a flat arch to be showed that he might conceivably have drawn a flat arch. His hand drew a flat arch in the air. So that the flat arch existed for him as a sort of motor function. How he translated this into the idea contained in his final statement I cannot explain. It might, I think, be explicable on the lines of Dr Abbott's suggestion regarding continuity and interruption. If the curve had come to mean continuity in the change of direction of the eye (analogous to continuity in the change of direction of the hand), the eye may have gone continuously to the topmost hole, encountered interruption there, fallen back again, and finally crossed from the second highest hole on the left side to the second highest hole on the right without breach of continuity. This would at once suggest the lowering of the highest hole. This seems plausible, but there remains the difficulty of the restlessness of the eyes.

The patient "rapidly learned the letters of the alphabet and figures, and he will soon be able to read and reckon." Acting on the doctor's advice, however, he has not meanwhile carried this further, as he finds it exceedingly difficult to identify such small things as printed letters. This is due not merely to a necessarily imperfect power of discrimination in the early stages of his sight-experience, but also to the kind of spectacles he uses, which are not suited for near vision. "He could count accurately after he had looked at objects one by one, and seemed to derive much help in his calculations by pointing with his finger. Here again he seems to translate touch into vision, and to arrive at a perception of the whole through perception of the individual parts. He cannot take in things at a glance. He does not see the passers-by on the opposite side of the street quickly. He looks most intently and moves his head backwards and forwards, and from side to side, as if trying to get a view of them all round before he can make up his mind what he is seeing—in a room, however, he can distinguish things much more quickly." My own impression from observing him is, not that he pieces things together by a separate study of their parts, but that he goes on looking until he finds something which suggests the whole and is then satisfied at having recognised it. He sees buildings and other large objects best from above. This is probably due to his want of practice in 'filling out' visual signs, on account of his want of familiarity with the visible appearances of different types of buildings, etc. Except in the case of points and gleams of light, his range of distinct vision is not great[1]. "One of the things that gave him peculiar pleasure was looking at the face of a watch which he had borrowed from a fellow-patient. Within a day or two of his having got the loan of it he astonished me by announcing that he was able to tell the time. When I asked him how he had learned so quickly, he said that he did not understand the figures on the dial, but he had been told how to count the hours, and that each space between the 'black marks' meant five minutes."

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[1] Cf. below, p. 145.

"He was able to estimate size and distance more readily than might have been anticipated, although he says that he feels that if he were out of doors by himself he would be 'wandered.'" When he did leave the hospital, he was, as might have been expected, very much confused by the life of the streets and the variety of things he saw. "From the time he got out of bed after the operation he could guide himself with ease through a doorway and walk about on the level." When I saw him he would walk in a straight line to the door, holding his right hand slightly out until he grasped the handle. But he walked forward without the slightest hesitation. "He had considerable difficulty in ascending a stair, because the steps seemed so high that, to begin with, he raised his foot much higher than was necessary and, without meaning to do so, went up two steps at a time. Whenever he discovered his mistake he began to pay attention to the rise of each, and he has now no difficulty in estimating their height." In general things seemed larger than he expected; but the great extent of space did not impress him until he had left the hospital. "When he first looked out of a window on to the street and saw the pavement below" [from the first story], "he said he felt that if he had a stick he should be able to touch it." He did not have the feeling of Cheselden's patient that the objects he saw "touched his eyes." I tried him also with Raehlmann's experiment of looking through a tube at two objects at unequal distances; but he had great difficulty in coming to any conclusion as to which was the nearer. "He did not retain his faculty of moving easily about in the dark. Before the operation he could guide himself fearlessly through a ward without coming in contact with the beds or any other obstacle that might be in the way, but since he has been able to see he says he has lost all that feeling of confidence, and when his eyes are shut he is afraid to move, and is impelled to open them to ascertain where he is going."

Pictures were at first to him "mere masses of confused colour." On my first visit to him I showed him a large coloured picture of a man's head, on the cover of a weekly illustrated paper. After long scrutiny he recognised the nose and mouth, and thus knew that it represented a head. But he was completely puzzled by the hair and by a pair of spectacles on the nose. Of smaller pictures he could make practically nothing. "He cannot tell the photograph of one person from that of another." On October 23rd I showed him a small print of the hospital (taken from a photograph) on the cover of the annual report. After a time he said it was a building; but it seemed to him to be "queer." He then pointed to the gable of another building which appeared in the background. It was without windows and looked comparatively dim. He could not make out what it was, and when I told him it was another building, he found it difficult to understand why it should seem smaller and dimmer than the building in the foreground. He found it difficult to grasp the distinction between the foreground and the background of a picture. Franz's patient had exactly the same difficulty.

The evidences of aesthetic appreciation in the case of Carruth are slight. He is fond of lights and bright things generally. The colour red gave him at once a feeling of pleasure; but dark colours did not specially depress him. Yellow, however, had a remarkable effect upon him. "The first time he saw yellow he became so sick that he thought he would vomit." This feeling has never recurred. I do not know how this is to be explained, but there is doubtless some physiological reason for it. At dusk one evening in summer he was taken to the top of a hill near his native village, and he at once saw the lights of Paisley (several miles off), which his companion had not noticed, and the gleam of light upon a small lake at a considerable distance. When in Bradford last summer he saw a tank with gold-fish in a shop window and was so fascinated that, as he said, "he stood looking at it for twenty minutes and found it difficult to tear himself away." He has a vivid recollection of the pleasure with which he saw the stars on the first night after he left the hospital, and he was specially delighted with "a large star" which he saw low down in the sky. He did not take much aesthetic interest in human faces. He had great pleasure in seeing his mother's face; but he immediately asked if the marks he saw were wrinkles, and he was pleased that he had guessed rightly.

He is still a little slow in recognising small objects with which he is not perfectly familiar. On October 23rd a long pencil was held up before him. It took him rather more than ten seconds to decide what it was, and then he called it a pen.

(*b*) *Visualising, Association, etc.* When I saw him on May 4th he had no experience of visual images. He did not understand what I meant when I questioned him about them; but I suggested to him that he should try to recall objects he had seen, and when I saw him again on May 9th he was able to obtain visual images without difficulty. On October 23rd he could easily picture the starry sky, with the planet, which had specially attracted him, in its right place. He could also picture with great vividness the tank of gold-fish. When he had forgotten the particular place of an object in a room he could now sometimes discover it by visualising and thus seeing in his mind's eye the object in its place. During his blindness his only means of discovering the place of the object in such circumstances was to return to the room and grope for it. Names of places and things with which he was familiar also, in some cases, suggested visual images. Thus the name 'Bradford' immediately gave him a vision of the gold-fish. 'Time' and '3 o'clock' suggested no image; but 'clock' gave him the image of an eight-day clock. In his ordinary association of ideas there seemed to be nothing peculiar. In the blind state it took place mainly through sound (the sound of words), but also to some extent through touch and smell. Now it is often dependent on visual images, as well as suggestive of them. Similarly his thinking in the blind state was mainly word-thinking, while now it is to a great extent picture-thinking. This, of course, is largely due to his great interest in the visible world, which has led to a very rapid modification of his former system of knowledge and interpretation.

He had many dreams in the blind state, and he dreams even more frequently now that he sees. The dreams which he specially remembers are those which he regards as having been fulfilled. He is, as one might expect, very easily satisfied as to the fulfilment of a dream. He told me two of them—one which occurred when he was blind, and another which he had some time after the operation. In the first of these he dreamed that a horse in a grocer's van ran away, as the result of losing a shoe, and jumped over a bridge into a stream. The only imagery in the dream was aural and tactual. He knew that the horse had lost a shoe by the sound of its hoof on the road. He heard a man approaching and talked with him. He remembered the feel of the iron on the end of the shaft of the van, that of the leather of the harness, and that of the shoe which he picked up. The dream, he thought, was fulfilled by the actual running away of a horse and van shortly afterwards, which ended in the horse plunging over a fence. In his visual dream he saw a table spread for tea, with chairs, cups, plates, etc. He sat down and found, to his great disappointment, that there was no bread for him. The 'fulfilment' came when he was asked to supper the following evening and could not accept the invitation because he had another engagement! His dreams in general seem now to be mainly visual. He has no recollection of ever having dreamed a smell.

This is practically all the information I can give regarding Dr Ramsay's patient. I was a good deal disappointed in my last interview with him. I hoped to [illegible] that he had taken to some manual occupation, and that it would be possible to get information from him regarding the progress in his knowledge of things. But he has not been doing any sort of work, and has occupied himself with social engagements, addressing 'revival' meetings and meditating on what he shall say at these meetings. I tried to question him about his meditations, but he would not say much about them.

It seems better to allow the case to speak for itself than to attempt to draw general inferences from it. Some of the material may be useful to one or another psychologist who is following a special line of study. But I should like to draw attention to the quickness of inference, and the rapidity of accommodation to new conditions in the case of Carruth. This, I think, is clearly due to the fact that he had already a considerably developed experience, both cognitive and conative, and that consequently the new sight-experience was easily assimilated. The perceptions of the ball and brick and of the arch illustrated this. This facility of assimilation is much more easily intelligible on the hypothesis of 'acquirement of meaning' and 'complication' than on the 'atomic' or associationist view. A great part of Carruth's quickness was due to his power of rapid inference, which is a feature of highly organised knowledge. His inferring power is probably considerably greater than it would have been had he not been blind—supposing, of course, that other circumstances (*e.g.* his occupation in life, etc.) remained the same. Psychologically, a blind man in his circumstances lives in two worlds—the world as it would be if no one saw, and, in part at least, the world as the result of mental process in human beings who see. He is continually getting messages from this second world which he must somehow interpret, and he is continually being called to do things which it would never have occurred to him to do had there been no seeing persons around him. This involves constant practice in inference or interpretation, with facility and skill as its result. But many blind people show much less quickness and intelligence than Carruth does. This, however, is a negative confirmation of my argument. For I should attribute Carruth's intelligence to the variety in his life during the blind stage, its many points of contact with the social world of people who see. The record shows that he was engaged in various occupations, hardly any of which could be called merely mechanical. His work was mostly of the nature of direct co-operation with seeing people. On the other hand, consider the case of his sister, on whom Dr Ramsay operated on April 18th, and May 1st, and whom I saw on May 9th. She is 33 years of age. "She had never been able to discern objects, though she could tell day from night, distinguish shadows, and even recognise bright colours when she held them close to her eye." During her blind state "she was weak and timid, frightened to go about alone unless in places with which she was perfectly familiar." "She lived mostly indoors and assisted her mother in the general housework and in the care of the children. When out of doors she rarely came against obstacles, but she says that she was prevented from doing so by perceiving their 'shadow,' rather than by any consciousness of their presence through an increased development of the sense of hearing. She seems not to have cultivated either the auditory or the olfactory senses, but to have relied wholly on the sense of touch for information regarding the world around her." "She showed no initiative, but listlessly travelled along the groove in which she found herself placed. When 19 years of age she began to learn the Braille type, and in less than a year was able to read. She took no interest in this, however, and tells me she now never reads at all. At 23 she entered the Blind Asylum and wrought there as a machinist for ten years, doing ordinary work in an ordinary way." Her work in the Blind Asylum was as mechanical as possible—passing the covers of mattresses through a sewing-machine. The contrast between the results of the operation in her case and in that of her brother is remarkable. "Whenever the bandages were removed the patient said she could see. Unlike her brother, she expressed no wonder at what she saw, but she has great difficulty in describing her sensations in such a way as to convey any clear conception of them to another." "Although she is manifestly delighted at her ability to see she never trusts her sight, and always tries to handle an object before naming it. The more closely she is watched the more evident does it become that she really makes very little use of her sight, falling back on her sense of touch for information, just as she had been taught to do in the

Blind Asylum. She does not seem to be able to think through her eyes, if I may so express it." "She does not use her sight to gain increased knowledge of the objects she handles—her touch tells her all she wants to know about them." When I saw her on May 9th I could get no information of value from her; but the contrast between her and her brother impressed me greatly. The deficiency in her intelligence and interest, and her tendency to relapse into the world of the blind seems to me to arise from the narrowness and the mechanical character of her practical life. Her sight is of little use to her because she has few practical interests, and those she has can be quite well satisfied in the blind state. Her experience is too thoroughly stunted to grow under the new conditions.

The special character of Carruth's case ought also, I think, to warn us against supposing that the experiences of an intelligent adult restored to sight are the same as those of a very young child learning to see. The young child can have no such organised experience as a ground for its sight-perceptions any more than it can have a hardened, stunted experience like that of the woman patient. And it is impossible to eliminate from the record of such a case as Carruth's all that is due to his pre-visual experience.

FURTHER NOTE. On April 11th, 1904, Dr Rivers and I tested quantitatively Carruth's perception of certain illusions. Many of the tests failed owing to the imperfect development of his power of grasping lines and figures as a whole and to his difficulty in interpreting figures representing three dimensions. But in the cases of the Müller-Lyer illusion and that of the comparison of vertical and horizontal lines the results were essentially similar to those which appear in persons with normal vision. So far as we could ascertain he had no perception of stereoscopic effects; but this may be due to defective vision in one of the eyes.

At the same time we examined another patient of Dr Ramsay, a young woman who, at the age of 20, recovered vision in one eye as the result of an operation in September, 1903. She had been blind from infancy. Her perception of the Müller-Lyer illusion was similar to that of persons with normal vision. She said that on regaining her sight, she found large objects which she had not handled (*e.g.* carts and horses) to be larger than she expected, while small objects with which touch had made her familiar (*e.g.* household utensils) seemed smaller. She was employed as a cotton-winder, and the only difference which sight made in her work was that she could now do it more quickly. She could still do the cotton-winding with her eyes closed. In going about, her movements were those rather of a blind person than of one who sees. She could visualise imperfectly. She had no recollection of dreaming during her blind state; but she could now dream. Her dreams, however, were confused, without distinct visual images. She could visualise her home, a table in a room, etc. But the images were not clear, and she could picture people in the room, objects on the table, etc., only after she had thought of them as in the position in which she pictured them.

THE VARIATION OF THE INTENSITY OF VISUAL SENSATION WITH THE DURATION OF THE STIMULUS.

(Four Figures in the Text.)

BY W. McDOUGALL.

§ 1. *Review of results of other workers—the wide discrepancies between them—probable sources of error pointed out.*

§ 2. *Description of new method of determining the 'action-time' of light of any given intensity, i.e. the duration of its action upon the retina necessary for the production of sensation of maximum intensity.*

§ 3. *The action-time of white light varies, inversely with its intensity, from about ·2 to about ·03 second.*

§ 4. *When the duration of action of light is less than its action-time, the intensity of the sensation is directly proportional to the duration of the stimulus. This fact seems to suggest the true explanation of the Talbot-Plateau law.*

§ 5. *The action-time of the minimum perceptible light is about ·2 second. The bearing of this fact on the principles of construction of flashing coast-lights.*

§ 6. *Criticism of Kunkel's work on the action-times of coloured lights. His over-estimation of the action-times of green and blue lights, relatively to that of red light, probably due to neglect to eliminate the effects of excitation of the rod-apparatus. Red, green, and blue lights of equal intensities have action-times of equal or very nearly equal duration.*

§ 1. *Critical review of the methods and results of other observers.*

ON considering the phenomena of flicker and of fusion of rotating sectors, it seemed to me that in order to arrive at a full understanding of them we must first ascertain the least time during which a light of given intensity must act upon the retina in order to produce its full effect in sensation, *i.e.* in order to excite the sensation of the highest intensity that it is at all capable of exciting. This time I propose for the sake of brevity to refer to hereafter as the 'action-time' of the light-stimulus. It seemed necessary also to ascertain what proportion of its full effect is produced by light of given intensity acting upon the retina during any period of time less than its action-time, *i.e.* during any fraction of its action-time. On referring to the text-books I found that the accepted determinations were those of Prof. Exner[1] and Dr Kunkel[2]. Their methods and results seemed to me to be open to serious objections, and I therefore began a series of determinations by a new method. When my preliminary experiments had been made, a paper was published by Prof. Götz Martius[3] in which he reported new determinations of the action-time, differing widely from my own and still more widely from those of Exner and Kunkel. Since I believed myself able to point out in the procedures of these observers sources of error that seem to account for the wide differences between their results, and that seem to be avoided in my own experiments, I continued the work and obtained the results recorded below.

Two other researches dealing with this problem must be mentioned, one by W. Swan, formerly Professor of Natural Philosophy in the University of St Andrew's[4], the other by Prof. A. Charpentier. Swan's procedure would seem to have involved most of the principal sources of error that I have to point out in the procedures of Exner, Kunkel, and Martius, and it is not necessary to criticise it in detail because it led to the conclusion that the action-time is the same for light of all intensities, namely about $\frac{1}{10}$ second, whereas Exner's, Martius' and my own researches, though differing widely as to the absolute duration of the action-time, are thoroughly concordant in showing that the action-time varies inversely with the intensity of the light, *i.e.* is shorter the more intense the light. Charpentier's experiments[5] are so briefly reported that it is not possible to criticise them in detail. He states that the action-time is about $\frac{1}{8}$ second, and (if I have not misinterpreted the report) implies that it is the same for light of all intensities, and this, as in the case of Swan's conclusion, would seem to indicate some defect in the experimental procedure.

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[1] "Ueber die zu einer Gesichtswahrnehmung nöthige Zeit," *Sitz.-Ber. d. Wiener Akad.* 1868, Abth. II. Bd. 58, S. 601.

[2] "Ueber die Abhängigkeit der Farbenempfindung von der Zeit," *Pflüger's Arch. f. Physiol.* Bd. IX. S. 197, 1874.

[3] "Ueber die Dauer der Lichtempfindungen," *Beiträge zur Psychologie u. Philosophie*, Leipzig, 1902, Bd. I. Heft 3.

[4] "On the gradual Production of Luminous Impressions on the Eye and other Phenomena of Vision," *Trans. Roy. Soc. Edin.* 1849.

[5] *Compte Rendu de la Soc. de Biol.* 1887.

Exner's research was made with an apparatus and by a method devised by Helmholtz, and described by him on page 513 of the *Physiologische Optik*[1] (where the apparatus is figured) in the following words:—"One presents to the observing eye first a sharply-bounded white semicircle on a black ground; after a measurable brief period of time a wholly white ground of equal brightness is presented, and a little later the whole field is filled with black. But the observer retains an after-image in the eye, which corresponds in intensity to the last moments of the sensation. If the first-made impression [*i.e.* that of the white semicircle] has already reached its maximum, the second impression not yet, then the former image remains as a positive after-image. If both have passed the maximum, then the former has already fallen to a lower degree of intensity than the latter; the after-image [*i.e.* of the white semicircle] is negative. Between these lies a moment, in which the original figure [the semicircle] is visible neither as positive nor as negative image, but is entirely indistinguishable (ausgelöscht). Then the first impression must have already overstepped its maximum, and the second one not yet have reached it." And the interval between the beginning of the second stimulus and this moment of extinction of both stimuli is held to be the measure of the time required by light of this particular intensity to produce its full effects for sensation, *i.e.* it is held to be the measure of the action-time of

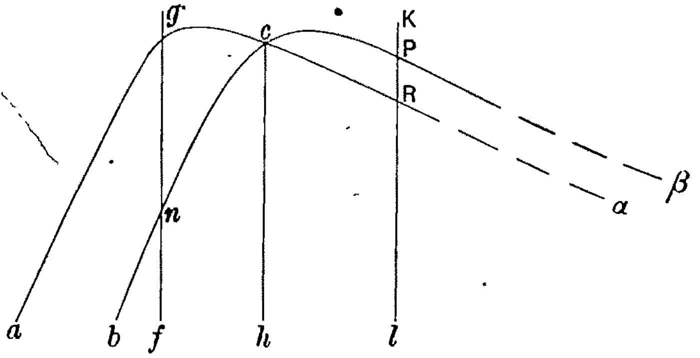

FIG. 1.

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[1] 2nd ed. 1896.

light of the given intensity. The principle and the assumptions underlying this procedure will be made clearer by an examination of fig. 1, which roughly reproduces a figure in Exner's paper. In this figure *a* and *b* represent the moments in which the two stimuli begin to act on the retina, the curves *aa* and *bβ* represent the assumed course of the intensity of the sensations produced, and *f*, *h*, and *l* represent points of time at which both stimuli may be simultaneously shut off; of these *h* represents the moment at which the curve *bβ* has attained, or very nearly attained, its maximum, and *bh* represents the time sought; if both stimuli are shut off at point *f*, the impression *aa* will be the more intense, but if at *l* the impression *aa* will have declined to a lower point of intensity than the impression *bβ*.

By experiments of this kind Exner arrived at the following results: using as his unit of intensity the light of a strong oil-lamp reflected from white paper and brought to the eye through a small telescope, and using stimuli of three lower grades of intensity of the value relatively to the first of ½, ¼, and ⅛ respectively, he found as the times of action of these stimuli necessary for the production of the maximal sensations the following figures:

Stimulus of value	1	requires	·1508 sec.
,,	½	,,	·2000 sec.
,,	¼	,,	·2460 sec.
,,	⅛	,,	·2873 sec.

On this and other similar series Exner based the general statement that, when intensities of the stimuli increase in geometrical progression, the times that intervene between the beginning of the stimulation and the highest intensity of the resulting sensation, diminish in arithmetical progression. By using, in place of two sources of light of equal intensity, two sources, one of which was given intensities equal to various known fractions of the intensity of the other, Exner believed that he was able to determine the form of the curve representing the rise of intensity of a sensation excited by a stimulus of given intensity, and in this way arrived at the curve reproduced on p. 515 of the *Physiologische Optik.*

This ingenious procedure is, I take it, physically admirable, but when physiologically and psychologically considered it seems open to the gravest objections. Martius has raised certain objections to it which, however, I do not regard as well founded. It was assumed by Helmholtz and by Exner that the curve of sensation-intensity begins sensory impressions, the process consists essentially in a turning of the attention from one impression to the second, the feeling of difference, or of more or less, being given immediately to consciousness as the attention is turned to the second impression. If the two impressions are of momentary duration and simultaneously presented the turning of the attention from one to the other becomes impossible, and therefore no accurate comparison can be made. The importance of these considerations was brought home to me by certain preliminary experiments in which I caused two momentary stimuli of slightly different duration to be applied simultaneously to the retina. I then found that a difference of intensity of the resulting sensations, that was easily and certainly appreciable when the two stimuli were applied successively at an interval of about one second, was quite inappreciable when two similar stimuli were simultaneously applied. In this connexion it is to be noted that Exner sought merely that duration of the later-beginning stimulus which just sufficed to abolish the perception of the other impression as a brighter or darker half of the circular field, which appearances he regarded as positive or negative after-images, respectively; and Exner did not claim to see these 'after-images' distinctly; he wrote, "This negative after-image is, as may easily be understood, not very distinct; it appeared always in the case of the objects of my observations as a brighter vertical stripe, corresponding to the boundary between the black and white semicircles which, owing to contrast, was especially prominent." It is noteworthy, too, that Kunkel, who later worked with an apparatus similar to Exner's, could obtain no results by Exner's method of procedure, because he failed to detect those differences of brightness of the two halves of the field, upon which Exner relied. (3) Exner's method is subject to a third important source of error, in that at the time at which the research was undertaken we were ignorant of the fact that our achromatic sensations have two physiologically independent sources, namely the rods and the cones. This source of error will be discussed in reviewing the experiments of Kunkel on the action-times of light of different colours, in which it seems to have played a more important part. (4) Exner seems to have confused together two problems, which it is important to distinguish clearly, the problems namely:—(*a*) How long must light of given intensity act upon the retina in order to produce the most intense sensation that it is capable of producing? (*b*) At what interval of time after the moment of incidence of the light upon the retina does the sensation excited by it reach a maximum? Exner seems to have regarded these two periods as necessarily identical, and to have aimed at determining the latter period of time, believing that in so doing he would determine the former. Now there is no necessary connexion between the two periods and, while the former period may be determined by suitable experiment, I venture to think that the determination of the latter is a problem that must remain for ever beyond our reach. The former period is a small fraction of a second, while the latter may be, and probably is, considerably longer. Helmholtz and Exner seem to have made the unjustifiable assumption that the course of the sensation, or the curve representing the rise of the sensation to its maximum intensity, coincides with the curve representing the rise of the physiological excitation immediately determined by the light acting on the retina. This assumption seems to underlie the curve on page 515 of the *Physiologische Optik* which, it is said, "die Empfindungstärke als Function der Zeit darstellt." This curve, based upon Exner's experiments, represents the sensation-intensity as rising with gradually diminishing rapidity throughout a period of nearly 200 σ.

I may be mistaken in attributing this assumption to Exner and Helmholtz, but there can be no doubt, I think, that they did not sufficiently distinguish between the course of the physiological excitation and that of the resulting sensation. That the curve representing the course of the physiological excitation rises gradually during many thousandths of a second is shown both by Exner's work and by the experiments to be described below; but that the sensation does not rise thus gradually, but much more suddenly, seems to be proved by the simple observation of a disc having an open sector and rotating at about two revolutions per second before an evenly-illuminated surface. In the case of a very dimly-lit surface it may then be observed that, while the eye remains at rest, the leading edge of the open sector, *i.e.* of the moving image, is not sharply defined, but through an angle of about 2° increases in brightness. This angle of 2° represents a period of about 11 σ, and that would therefore seem to be the length of the period during which the sensation rises in intensity. If now the illumination is increased, this small region of increasing brightness becomes rapidly narrower until, when the intensity of the stimulus is of the same order as that dealt with in the case of the curve given by Helmholtz, its width is so reduced that it is not certainly perceptible (it is certainly much less than 1° in width), and the sensation seems to attain almost instantaneously its maximum intensity; whereas, if

Helmholtz's curve truly represented the facts, we should see the intensity of the sensation increasing throughout an angular width of about 36° from the leading edge backwards[1]. These deficiencies of Exner's method are, I think, serious enough to render the figures obtained by means of it entirely unreliable.

Although Martius' criticisms of Exner's method do not seem to me to be well founded, it must be admitted that Martius' procedure is superior to Exner's, but since his results differ from mine (the times he finds being about as much too brief as Exner's are too long, according to my determinations) it is necessary to point out what seems to me the source of error in his work.

Martius' apparatus consists essentially in a duplication of the apparatus used by Exner, with various additional complications which secure increased nicety in the physical conditions of the experiment. By means of this apparatus two fields are presented by two telescopes, of which the eye-pieces lie a few centimetres apart. In the one telescope an image of a given intensity is continuously present, in the other a field of the same intensity is presented during a small known fraction of a second at regular intervals of some seconds. The observer looks first through one telescope and then through the other, and compares the brightness of the momentarily-enduring image with that of the constant image, and the duration of the former is increased until it appears equally bright with the latter; this duration is then taken to represent the time sought, *i.e.* the time during which the light must act upon the retina in order to produce its full effects in consciousness or its action-time. By this procedure Martius found the times of action of lamp-light of each of six intensities necessary to produce an impression of brightness equal to that produced by a continued image of the same intensity in each case. Of the brightness of these lights we are told "Immerhin möchte die Intensität J_1 nicht viel unter der einer gewöhnlichen Lampenbeleuchtung liegen[1]," and the other five lights were of intensity $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$ and $\frac{1}{32}$ of J_1. His results in tabular form are

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[1] The truth seems to be that the stimulus applied to the retina excites a physiological activity which increases for some time in intensity before any psychical effect is produced, and though this period is not measurable its duration may be compared in the case of stimuli differing in intensity and quality. In a former paper (this *Journal*, Pt I. p. 105) I have shown that this period varies but very slightly in the case of sensations excited by stimulation of the cones, being a few thousandths of a second less in the case of a bright light than in the case of a dull light, and is about 55 σ longer in the case of sensations excited by stimulation of the rods. These facts are completely in accordance with the hypothetical scheme of the physiological events that I have proposed. In terms of that scheme I would say that the stimulus to the retina effects a liberation of energy in the sensory neurones, which liberated energy or 'neurin' accumulates in the sensory neurones until it has reached a certain pressure or potential sufficient to overcome the resistance of the synapses of the cerebral cortex, when it discharges suddenly and almost instantaneously across those synapses, exciting in so doing a sensation of intensity corresponding to the quantity of energy discharged.

It is perhaps worth noting in this connexion that the action-time found by Exner for the feeblest of the four intensities used by him, namely 287·3 σ, is considerably greater than the simple reaction-time to a visual stimulus, and it would follow, from the above considerations, that, if Exner's figures are accurate, the sensation excited by a stimulus of this intensity must follow the completion of the reaction at an interval of nearly ·1 second.

J_1	requires	$13\,\sigma$	$J/8$	requires	$66\,\sigma$
$J/2$	„	$39\,\sigma$	$J/16$	„	$78\,\sigma$
$J/4$	„	$53\,\sigma$	$J/32$	„	$93\,\sigma$.

And he infers that still brighter lights than J_1 would require still briefer periods proportionally, periods of the order of about $1\,\sigma$ or less.

Now this procedure demands of the subject a real comparison of the intensities of the two successively-presented sensations, and in so far is sound. But there are two serious drawbacks to it, which, I think, sufficiently account for the discrepancy between Martius' determinations and my own. In the first place, it is very detrimental to accurate comparison to move the head and the eye from one eye-piece to the other, for this must involve a considerable interval of time and various movements of the eye and readjustments of the accommodation and so forth. Accurate comparison of intensities of visual sensations is, as anyone who has attempted it knows, difficult under the most favourable conditions, and the interposition of this period of time occupied by these movements and ocular adjustments cannot but seriously diminish the accuracy of the comparison. But a more serious objection seems to me the fact that the one image is constant in character, *i.e.* is of unlimited duration. Martius rejects the view of Exner and Helmholtz that the sensation declines at once after reaching its maximum. But in this he is undoubtedly in error. Charpentier has pointed out that as a white image traverses a dark field of view its leading part, of a width corresponding to a duration of about $\frac{1}{60}$ sec., appears distinctly brighter than the succeeding parts of the image; and I shall describe below (p. 180) experiments which prove in another fashion the continuous decline of the intensity of the sensation (produced by continued stimulation), a decline beginning a few thousandths of a second only after the maximum intensity has been reached. Now it is probable that in Martius' experiments the observer would fixate the constant image for at least a second, perhaps for a longer period; but during one second the intensity of the sensation declines very considerably, and though the subject remains unaware of this decline, his estimate of the brightness of the light is much affected by it, as my experiments show. This is, I think, the explanation, in part at least, of the fact that Martius finds a very much briefer action-time than is given by my observations, in the case of lights of about the same brightness.

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[1] *Op. cit.* S. 328.

§ 2. *The action-time determined by a new method.*

The above considerations have determined the choice of my own method. My procedure was as follows:—A large disc, 75 cm. in diameter (*D* in figs. 2 and 3), consisting of two apposed sheets of stout millboard, supported upon a smoothly-running axle, rotates in a vertical plane, in such a position that the ray from a projection-lantern (*L* in fig. 3) falls upon its outer border at right angles to the plane of the disc. In the outer border of the disc two radial gaps are cut at opposite extremities of one diameter, either gap is 8 cm. in radial length and 40° in width. The width of either gap can be diminished to any required extent by moving a sector of thin black card 40° in width, which lies

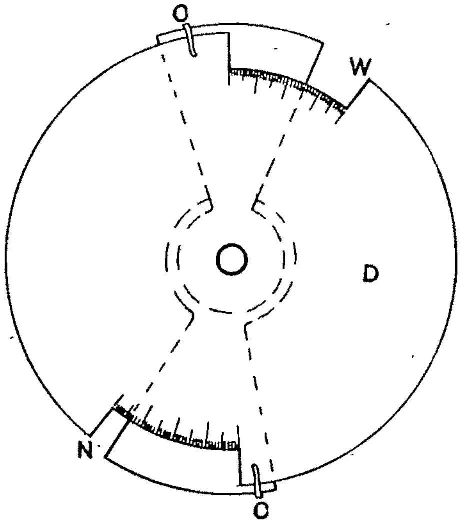

FIG. 2.

between the two sheets of millboard, is pierced at its central end by the axle and is held in position peripherally by a spring-clip (fig. 2 (*C*)) that embraces the edges of the two sheets of millboard. The edge of either gap is marked with a scale of degrees from 1° to 40°, and when the spring-clip is removed, either sector can be easily slipped to and fro between the millboards until its edge lies upon any point on this scale of degrees. In this way the width of either gap may be readily varied from 0° to 40°. As the disc rotates it interrupts the ray of the lantern except during the time of passage of the two gaps. The ray passing through either one of the gaps falls upon a thin sheet of milk-glass (*l* in fig. 3) fixed in a large vertical screen (*S* in fig. 3) parallel to the plane of the disc, 10 cm. from it and 50 cm. from the anterior lens of the lantern.

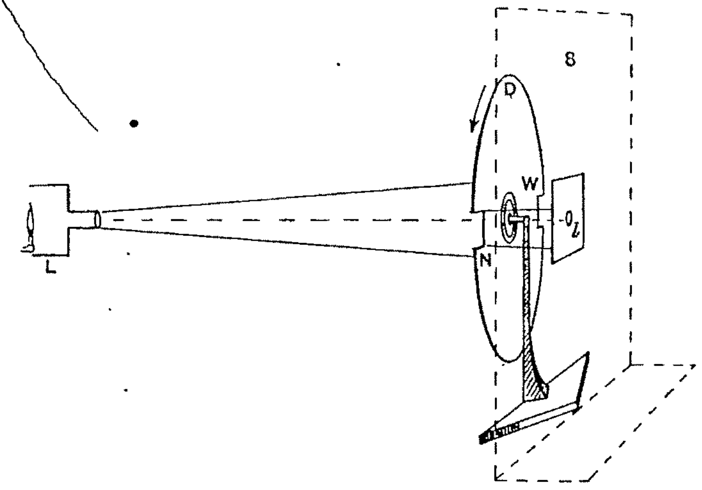

Fig. 3.

Over the plate of glass, on the side remote from the lantern, is fixed an iris-diaphragm concentric with the circle of light projected from the lantern, and on the other side of the plate is a sheet of opaque black paper having a circular hole 8 mm. in diameter which occupies the centre of the circle of light, *i.e.* is opposite to, or concentric with, the iris-diaphragm. As the disc rotates, the shadow of the posterior edge of the gap travels across this small exposed area of the milk-glass, which then remains completely obscured until the leading border of the other gap comes round, when it is again illuminated for a period that is determined by the width of the gap and by the rate of rotation of the disc.

The glass plate is supported in a large vertical screen standing upon the table, and the observer sits before this screen, *i.e.* on the side remote from the lantern and facing it, his chin supported upon a rest in such a position that his right eye is 50 cm. distant from the glass plate and directly in the line of the ray of light projected upon the circular exposed area of the glass. As the disc rotates he then sees this small circle momentarily illuminated during the passage of either gap in the border of the disc[1].

The disc is kept in regular motion by a clockwork driven by a heavy weight and controlled by a very delicate pair of wind-sails. In order to render these rapidly rotating sails more delicately responsive to any change of rate, they are bound together by a stout band of indiarubber, which is put on the stretch as the sails fly out under the influence of centrifugal force. The pulley-surface of the clockwork transmits its motion to a transmitter by means of a well-fitting strap, and this in turn drives the disc by means of a strap running over a wooden pulley on the axle of the disc 12 cm. in diameter. The rate of revolution of the disc in most of my experiments was one revolution in two seconds, and at this rate the large, heavy and carefully-balanced disc has a very considerable momentum which affords a further guarantee of regularity of rate of movement. The rate of rotation was determined before and after each series of experiments by timing thirty or more revolutions with a stop-watch, and it was not found to vary appreciably.

The two gaps being made of unequal angular width, the narrower (which hereafter will be referred to as *N*) being 6°, and the wider one (hereafter referred to as *W*) being 12°, and the disc set in motion at 1 revolution per 2 seconds, while one burner of the lamp gives a steady and constant flame, the observer sees the small circle of the glass plate, left exposed by the iris-diaphragm, momentarily illuminated at intervals of 1 second;

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[1] Owing to the kindness of Dr Waller I was able to make one series of experiments using an arc-lamp. In that case the border of the disc was made to rotate through the anterior focus of the lamp. In this way all parts of the field were simultaneously illuminated and obscured. But my work was chiefly done where there was no electric current at my disposal and the source of light I used was an acetylene lamp with four burners, closely set in a row and backed by a concave reflector. With this source of light it is of course impossible to obtain a very small anterior focus, I therefore adopted the arrangement described above. The disc I used was so large that the rate of movement of its outer border was considerable and the eye could not appreciate the fact that the shadow or the illumination advanced across the small field, and I think that the error, if any, introduced by this fact is very minute. The results obtained with the two arrangements are quite concordant when allowance is made for the greater intensity of the illumination in the experiments in which the arc-lamp was used.

and its alternate appearances are distinctly darker and brighter. The observer then, by rising and leaning forward, can look round the edge of the vertical screen which carries the glass plate and by a glance at the disc can see that the duller flashes are due to the passage of the narrower gap, *N*, while the brighter are due to the passage of the wider gap, *W*; *i.e.* of the two stimuli of equal physical intensity the more prolonged excites the more intense sensation, and hence it may be inferred that the duration of the briefer stimulus is less than the action-time of light of this intensity.

The width of *W* was then reduced in successive steps and the observation repeated at each step, in order to determine the smallest amount of difference in width of *N* and *W* that gives an easily and certainly appreciable difference in brightness of the two flashes. It was found that when *N* was 6° and *W* was 8° in width, the flash given by *W* was still perceived to be brighter than the flash given by *N*, with certainty so long as the attention was concentrated upon the relative intensity of the sensations; but that when the difference of width was made less than 25 °/₀ of the width of *W*, the difference of alternate sensations was no longer easily and invariably perceived.

I proceeded therefore to increase the width of both slits, keeping them always in the proportion of 3 to 4 or very nearly so, and increasing the width of *N* by 1° after each observation. It then appeared that while *N* was 6°, 7° or 8°, the greater brightness of the sensation yielded by *W* was distinct, that when *N* was 9° and *W* 12°, the difference became less marked, that when *N* was 10° and *W* 13½° there was no certainly distinguishable difference of brightness, and that when *N* was 11° and *W* was 15° there was no perceptible difference of brightness of the alternate appearances. I concluded therefore that the time of passage of 11° of the disc past any point is approximately the time during which a light of this physical intensity must act upon the retina in order to produce its full effects in consciousness, to excite a sensation of the highest intensity that it is at all capable of exciting under the given conditions; or more shortly stated, I concluded that the action-time of light of this intensity falling upon the fovea is approximately 61 σ ($\frac{11}{360}$ of 2 seconds).

I then proceeded to make further steps of the same kind and found that when *N* was 13° and *W* 17½°, a difference of brightness again appeared and persisted when the series was further prolonged in the same direction. But the brighter impression was in these cases excited by the passage of *N*, *i.e.* by the briefer exposure. This result was quite unexpected by me and I was for a time puzzled to account for it. It appears quite constantly when the light is of fair intensity, but less certainly when the light is very dull.

In order to study this effect I then kept *N* at 11° and in successive experiments increased the width of *W* by 2°. At each step in this series the apparent difference of brightness became more marked until *W* reached the maximum width of 40°, giving an exposure of a quarter-of-a-second duration. There will be found below (p. 180) the report of a few experiments by which I sought to measure the degree of this apparent decline of brightness of the sensation with increase of duration of the action of the light on the retina beyond the action-time of the light. From those experiments it appears that when a light of this intensity acts for a quarter of a second upon the retina the resulting sensation appears no brighter than that which results from a light nearly 40 % less bright falling on the retina during its action-time only.

There can be no doubt, I think, that this apparent loss of brightness of the whole impression is due to that decline of the curve of sensation-intensity, beginning very soon after the curve reaches its maximum, which was postulated by Helmholtz and Exner as the basis of their method. For although the observer cannot distinguish successive parts of the sensation enduring for one-quarter of a second only, yet if during this brief period a considerable decline of the intensity takes place, the total impression, compared with that resulting from a stimulus of equal intensity that is cut short before such decline has occurred, may be expected to be that of a duller light and such, as we see, seems to be the case.

This apparent loss of brightness on prolongation of the stimulus beyond its action-time renders my method more accurate than it otherwise would be, because it enables me to find a narrow range of durations within which the action-time must lie, a range of which the lower limit is definitely marked by the distinctly brighter appearance of the longer flash, and the upper limit by the distinctly brighter appearance of the briefer flash.

In order to determine as accurately as possible that duration of *N* at which its flash ceases to be distinguishable from *W* in a series in which the duration of both *N* and *W* is increased step by step in the manner described above, it is clearly desirable that the observer should not know, until after expressing his judgment, which of the sensations is given by the longer and which by the shorter flash, or in technical phrase, the procedure should be without knowledge on the part of the subject, as far as possible; and since the experiment aims at determining the threshold of difference for the two impressions, it is advantageous to apply one or other of those psycho-physical methods which have been worked out and successfully applied to the determination of thresholds of difference in so many other cases. After some preliminary experimenting I chose that method which I have previously applied to the determination of the delicacy of tactile discrimination[1] and which consists in a combination of the method of minimal changes with the method of right and wrong cases.

The conditions being the same as in the series above described (*i.e.* the disc revolving at 1 revolution per 2 seconds, the glass plate illuminated by the full power of one burner of the lantern, and a circle of it, 7 mm. in diameter, exposed through the iris-diaphragm), I proceeded as follows:—*N* was made 7° and *W* 9·5° in width and the disc set in motion. Taking my seat before the screen, which hides every part of the disc, placing my chin upon the rest and covering my left eye with my left hand, the left elbow resting on the table, I kept my right eye steadily directed to the little circle of light which made a momentary appearance at intervals of one second, and continued to compare the brightness of the successive impressions until I clearly perceived the alternation of bright-dull, bright-dull. Frequently I did not perceive the difference of brightness of the two flashes first seen, and often did not feel certain of the alternation of brighter and duller flashes until they had been repeated several times. For the perception of the small difference of brightness of the momentary flashes demands an accurate accommodation of the eye and a favourable setting of the expectant attention for each flash[2]. The repetition of the flashes therefore was, not only no drawback, but very advantageous, and no error could be introduced by any slight fatigue, as might have been the case if only the two flashes of slightly different duration had been presented, for any such fatigue must affect equally the apparent brightness of the alternate flashes.

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[1] *Reports of the Cambridge Anthropological Expedition to Torres Straits*, 1903, vol. II. p. 190.

[2] The importance of the setting of the attention was well shown if I attempted to prolong unduly a series of observations of this sort. The process is wonderfully fatiguing, and continued work very rapidly reduced me to a condition in which I began to feel the effort of attention to be very great, and then to a further condition of fatigue in which it was impossible sufficiently to prepare the attention for each flash. Perception of difference was then impossible.

When in this way I had arrived at a clear perception of the alternation of bright and dull flashes, I leant forward and by a glance round the edge of the screen learnt which of the two gaps in the border of the disc, *N* or *W*, corresponded to the brighter impression. I then closed my eyes for a few seconds during which I lost the count of the flashes, and then observed them once more until I again perceived clearly the alternation of bright and dull.

This procedure was repeated six times with *N* and *W* at the same width, namely 7° and 9·5° respectively, and again six times after each increase in the width of *N* and *W*. The results of the series of observations were written down in the following form:—

N	7°	0	*C.*
W	9·5°	6	
N	8°	0	*C.*
W	11°	6	
N	9°	1	*F. C.*
W	12°	5	
N	10°	1	*F. C.*
W	13·5°	5	
N	10·5°	2	*N. C.*
W	14°	4	
N	11°	3	*N. C.*
W	15°	3	
N	12°	4	*N. C.*
W	16°	2	
N	13°	5	*F. C.*
W	17·5°	1	
N	14°	6	*C.*
W	19°	0	

Table 1.

This table presents the results of a complete series of observations with light of one intensity, namely that of one burner at full power transmitted by the plate of milk-glass. *N* and *W*, with the figure standing after either, denote the narrower and wider gaps and their angular width at each step of the series; the figure standing last in each line denotes the number of times that the impression made by the passage of *N* (or *W*) was judged to be brighter than that made by the passage of *W* (or *N*). The letters *C.*, *F.C.*, and *N.C.* stand for confident, fairly confident, no confidence, respectively, and express the feeling of the observer as to the reliability of the judgments recorded. When *N.C.* was written it was because I was aware that my judgment was little more than a guess. It might be supposed that when the impressions *N* and *W* were of equal intensity it would be impossible to arrive at a judgment of inequality, but such was not the case. I found that the setting of the expectation sufficed then to cause either one to appear brighter, so that by voluntarily setting the attention for the alternation bright-dull, either N or W could be made to appear slightly brighter. Hence the necessity for a systematic procedure of the kind adopted.

From this table it is clear that the action-time of light of this intensity is greater than the time of passage of any point on the disc through 10° ($55\,\sigma$) and less than the time of passage of any point through 13° ($72\,\sigma$). We may accept the duration of N at which the difference between the two flashes first becomes imperceptible as the action-time of light of this intensity, though theoretically this duration must be very slightly less than the full action-time. We may then take the time of passage of any point of the disc through 11°, *i.e.* 61σ, as the action-time of light of this intensity.

I have repeated this series of observations six times at intervals of some weeks and the extreme limits of the results were $55\,\sigma$ and $66\,\sigma$. The accuracy of the result is further guaranteed by the fact that the figure found ($61\,\sigma$) for the action-time of light of this particular intensity falls into its natural place in the series of figures expressing the action-times of lights of different intensities, as described in the following section.

§ 3. *The variation of the action-time with the intensity of the stimulus.*

We have seen that, while Swan stated that the action-time is the same for lights of different intensities, Exner and Martius agree in stating that it diminishes considerably with increase of intensity of the light. I have therefore determined by the above-described method the action-time of lights of twelve different intensities, each of which has approximately one-half the intensity of its predecessor in the descending scale of intensities. The light of highest intensity, the unit intensity of this scale, L, was the light transmitted to the eye by the plate of milk-glass when illuminated by the light of the four burners of my lamp in place of the one burner of the experiments described above. A light of approximately one-half the intensity of L, or $1/2L$, was obtained by using only two burners, and $1/4L$ by using one burner only. The lower intensities of the series were obtained by using in all cases as source of light the flame from one burner and interposing in the path of the ray of light as it leaves the lantern one or more 'smoked' glasses.

From a considerable collection of 'smoked' glasses I chose three similar glasses each of which transmits $\frac{1}{2}$ of the light, one which transmits $\frac{1}{4}$, and one which transmits $\frac{1}{16}$ of the light. These glasses were selected and the proportion of light transmitted by them was determined by means of an episkotister[1]. A strip of white paper 1 cm. wide and 10 cm. in length was pasted horizontally on a black vertical surface. The episkotister was then fixed in such a position that the eye saw one-half of this strip through the rapidly rotating episkotister, the other half through the glass of which the value was to be determined. The width of the open sectors was then adjusted by the method of minimal changes until the two halves of the strip of white paper appeared equally bright.

By combining these five glasses in various ways it was then possible to obtain the remaining steps of the scale of twelve intensities. The action-time of light of each of these twelve intensities was determined by a series of observations similar to that described above, and the whole series of series was twice repeated with but little variation of the results. The following table gives the action-times so determined in that one of these three series that seemed the most satisfactory because it was the last made and because therefore, when making it, I enjoyed the advantage of having had very considerable practice in this kind of observation.

			Differences	
Action-time	of	$L = 49\,\sigma$		
»	»	$1/2\,L = 55\,\sigma$	$6\,\sigma$	mean = $5{\cdot}6\,\sigma$
»	»	$1/4\,L = 61\,\sigma$	$6\,\sigma$	
»	»	$1/8\,L = 66\,\sigma$	$5\,\sigma$	
»	»	$1/16\,L = 78\,\sigma$	$12\,\sigma$	mean = $11{\cdot}3\,\sigma$
»	»	$1/32\,L = 89\,\sigma$	$11\,\sigma$	
»	»	$1/64\,L = 100\,\sigma$	$11\,\sigma$	
»	»	$1/128\,L = 127\,\sigma$	$27\,\sigma$	mean = $16{\cdot}6\,\sigma$
»	»	$1/256\,L = 142\,\sigma$	$15\,\sigma$	
»	»	$1/512\,L = 150\,\sigma$	$8\,\sigma$	
»	»	$1/1024\,L = 183\,\sigma$	$33\,\sigma$	mean = $25\,\sigma$
»	»	$1/2048\,L = 200\,\sigma$	$17\,\sigma$	

Table 2.

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[1] This instrument, constructed by Zimmermann of Leipzig, consists of a disc made up of two sectors each 20° wide, firmly attached to the axle and bearing a narrow rim marked with the scale of degrees. 18 sectors, each of 20° in width, borne upon the same axle, can be clamped in any desired position, *i.e.* overlapping one another to any desired extent, leaving open sectors of any required width from 0° to 320°. When this is in rapid rotation it transmits, of course, of the light projected upon it an amount exactly proportional to the width of the open sectors.

The determination of the action-times of the lights of the lower part of this table is less accurate than in the case of the lights of higher intensity, because discrimination of difference of these feebler intensities is more difficult, and because the reversal of the relative brightnesses of N and W, when the duration of N exceeds its action-time, does not occur with lights of intensity less than about $1/32L$, and therefore in these cases the higher limit of the range of possible error is not definitely fixed as it is in the case of the brighter lights. Nevertheless the fairly regularly increasing rate of increase of the action-time throughout the series shows that the errors are not very large.

The question arises, Can we infer anything as to the probable action-time of still brighter lights than L from a consideration of this table of results? It is clear that the rate of diminution of the action-time with increase of intensity of the light diminishes as we ascend the scale of intensities. There can be little doubt that this diminution of the rate of diminution of the action-time would be found to be perfectly regular if by any means we could determine the action-times of the lights of different intensities with absolute accuracy. This diminution appears most clearly if we minimise the irregularities, resulting from unavoidable errors of observation, by putting together the differences in groups of three and taking the mean of these groups. We thus get the figures of the last column of the table, namely $25\,\sigma$, $16{\cdot}6\,\sigma$, $11{\cdot}3\,\sigma$, $5{\cdot}6\,\sigma$. From this we may infer with some confidence that the rate of diminution of the action-time continues to diminish in the case of lights of still higher intensities than L of the above series, that, for example, the action-time of light of intensity $8L$ would be less than that of light of intensity $4L$ by about $1\,\sigma$ only; and that with still further increase of intensity the diminution of the action-time would become vanishingly small.

Martius has inferred by a similar process of reasoning from the results of his observations that the action-time of light of such intensity as the eye commonly receives in the ordinary course of life is very small. He writes "Die im täglichen Leben wirklich vorkommenden Helligkeiten haben also keineswegs eine lange Zeit des Anklingens nötig. Sie wirken fast unmittelbar." And he states that one of his pupils working by his method has found the action-time of a light of higher intensity than any he himself had worked with to be $4\,\sigma$ only. Now Martius' results, if we could accept them as accurate, would undoubtedly justify the inference that the action-time of very bright light must be very minute, probably much less than $\cdot 1\,\sigma$. For he finds that, according to the results obtained by his method, the rate of diminution of the action-time increases with increase of the intensities; thus

$$\begin{array}{ll}
J_1 & \\
 & \text{difference} = 24\,\sigma \\
J/2 & \\
 & \quad\text{,,} \qquad = 19\,\sigma \\
J/4 & \\
 & \quad\text{,,} \qquad = 13\,\sigma \\
J/8 & \\
 & \quad\text{,,} \qquad = 11\,\sigma \\
J/16 & \\
 & \quad\text{,,} \qquad = 10\,\sigma \\
J/32. &
\end{array}$$

But the fact that his observations lead to this conclusion affords I think a further proof of the inadequacy of the method employed by him. For, in the first place, it is obvious that this rate of diminution of the action-time cannot continue to increase in the same way with further increase of brightness of the light, for, if that were the case, since the action-time of J_1 is found to be 13 σ, a light of intensity twice as great would require less than no time to produce its full effects, which is absurd. And it is clear that with further increases of the intensity of the light beyond J_1 the diminution of the action-time must proceed at a rapidly diminishing rate (as it does throughout, according to my results) instead of at the increasing rate found by Martius for the intensities ranging from $J/32$ to J_1. That there should be any such reversal of the law of the rate of diminution of the action-time with increasing intensity of the light, at any point in the scale of intensities, is exceedingly improbable *à priori*, especially improbable is it that the brightness of this reversal point of the scale should happen to be just that of the brightest light (J_1) of Martius' series; and the fact that Martius' results would compel us to accept such a reversal, suffices, I think, to throw very serious doubt upon the adequacy of his method of experimenting.

Secondly, although it is not possible to measure accurately the action-time of light of much higher intensity than my L_1, because the eye can no longer compare with accuracy lights of so high intensity, yet it is easy to show by the following experiment that the action-time of such light exceeds a certain duration. I replaced the sheet of milk-glass in my apparatus by a sheet of ground-glass (roughened on one surface only). The diffusion of the light by such a glass is very imperfect, and, when the ray from four burners is projected upon this by the lantern and the eye is in the axis of projection of the lantern, the circle of light appears dazzlingly bright, far too bright to allow of discrimination of small differences of intensity. But if the gaps N and

W of the disc are made 5° and 10° in width respectively, then when the disc rotates at 1 revolution per 2 seconds, the alternate flashes appear very distinctly of unequal brightness, the less bright corresponding to N. The duration of the flash N is in this case about $28\,\sigma$ and the action-time of this very bright light is therefore considerably more than $28\,\sigma$. It seems probable then that the action-time of even direct sunlight is not less than about $20\,\sigma$ or $\frac{1}{50}$ second.

Thirdly, it is possible, I think, to point out the probable source of the error (as it seems to me) on which is founded Martius' inference as to the very brief duration of the action-time of bright light. In the general criticism of Martius' method on p. 159, I pointed out that the brevity of the action-times found by him is probably to be explained by the fact that the light with which he compared the momentarily exposed light was of indefinite duration and that the observer presumably fixated it for some considerable fraction of a second or perhaps more than 1 second, whereas the brightness of the impression begins to decline rapidly very soon after the action-time has been exceeded. Now, since this decline must be due to fatigue of some part of the retino-cerebral tract, it is highly probable that the decline of the sensation-intensity is greater and more rapid the brighter the light used, for the more intense the stimulus the more marked must be the effects of fatigue and the more rapidly will they manifest themselves.

Lastly, this conclusion is justified by a comparison of his results with my own as stated above. Martius states that "Die benutzten Lichtstärken sind mithin durchweg geringer als die gewöhnlichen Tageshelligkeiten, selbst bei bewölktem Himmel, anzusehen....... Immerhin möchte die Intensität J_1 nicht viel unter der einer gewöhnlichen Lampenbeleuchtung liegen[1]."

In order to render my results comparable to those of Martius and of other observers I have roughly determined the intensity of my light, $1/4L$, in the following way. The circle of the milk-glass plate 7 mm. in diameter being illuminated by the flame of one burner of my lantern as in the above experiments I put beside it, on a ground of black velvet at a distance of 20 cm., a disc of white paper 7 mm. in diameter. This disc was illuminated by an ordinary oil-lamp with duplex burner, well trimmed and burning with its maximum power. The light of this lamp was prevented from falling on the circle of the glass plate by a vertical screen. When the lamp was brought within a few inches of the disc of paper this was very markedly brighter than the circle of brightness $1/4L$. The lamp was then removed step by step until the two white circles seemed of equal brightness. This was found to be the case when the lamp was 35 cm. from the disc of paper. It is clear then that my $1/4L$ was at least as bright as the J_1 of Martius and probably considerably brighter, and that my L was at least four times as bright as Martius' J_1. Yet Martius finds the action-time of J_1 to be $13\ \sigma$, while by repeated experiments I find the action-time of $1/4L$ to be $61\ \sigma$ and of L to be $49\ \sigma$.

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[1] *Op. cit.* S. 328.

There can then be no doubt, I think, that Martius' method leads to far too low an estimate of the action-time, and that his inference as to the action-time of bright light is still further from the truth.

It is perhaps worth while to attempt a rough comparison of the results stated above with those found by Exner. For although the criticisms of his procedure stated above show that the results cannot be accepted as reliable, it is yet possible that the various sources of error that were pointed out may have partially neutralised one another, or that the amount of error due to them may be small. The brightest light of Exner's series was the light of a powerful oil-lamp reflected from a sheet of white paper and transmitted to the eye through a train of lenses. It is difficult to compare this with the intensities of my scale, but it seems probable that it would be of about the same intensity as my $1/4\,L$, or as the J_1 of Martius' series. Exner estimates the action-time of light of this intensity to be $150\ \sigma$. Now this is, according to my determination, the action-time of a light of intensity $1/512L$ of my series. This is a very dim light, and there can be no doubt that it is very much less bright than the brightest of Exner's series of four lights. We may conclude, then, that owing to one or more of the sources of error noted above, Exner's method leads to an estimate of the action-time that is very much too high.

Here may be inserted the account of an observation which is of the nature of a supplement to my paper[1] in the preceding number of this *Journal*.

In that paper it was shown that a single brief stimulus to the eye, of not too low intensity, excites a multiple response, a rapid series of pulses of sensation of diminishing brightness. If the stimulus be other than very brief no such series of pulses is excited, the sensation simply persists for a moment and then ceases. The question arises, What is the limit of duration of the stimulus that excites the multiple reaction? and what is the relation of this duration to the action-time? The answer is given by the following observation.

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[1] "The Sensations excited by a Single Momentary Stimulation of the Eye."

The apparatus being arranged as before, and the circle having luminosity 1/4 *L*, *W* was made 30° and *N* 5°. Then on rotation of the disc at 1 rev. per 2 secs., the sensation excited by *W* exhibits no trace of a multiple character, while the sensation excited by *N* is distinctly multiple. (The multiple character was obvious throughout the experiments described above, although the conditions of these experiments are not the most favourable for the observation of this feature of the response.) The width of *N* was then increased in successive steps, 1° at each step. The multiple character of flash *N* as compared with flash *W* remained equally well marked until *N* was 14° wide. From this stage onward the multiple character of *N* became less marked, until when *N* was 17° wide it ceased completely, and flash *N* resembled in character flash *W*, save for its perceptibly briefer duration. Now we know from the previous determinations that the action-time of light of this intensity ($1/4L$) is 61 σ, corresponding to a width of 11° of *N*. The multiple character of the response persists, then, in the case of light of this intensity, until the duration of its action exceeds its action-time by about 16 σ, and ceases altogether when the duration of the action of the light exceeds its action-time by about 40 σ. This fact is fully in harmony with the explanation of the appearance of the multiple response that I have suggested in the paper referred to[1].

§ 4. *The variation of the intensity of the sensation with the duration of the stimulus when that duration is less than the action-time, and the interpretation of the Talbot-Plateau law.*

What is the intensity of the sensation excited by a light acting upon the retina for any given fraction of its action-time? This question can only be answered in terms of stimulus-intensity, *i.e.* we can express the intensity of the sensation produced only in terms of the stimulus-intensity. And it is clear that the light which is to give the measure of the intensity of the sensation produced must be allowed to act upon the retina for a period equal to, or only slightly greater than, its action-time; for if it be allowed to act for a longer or a shorter period the resulting impression of brightness will be of diminished intensity. A slight modification of the apparatus described above enables us to answer this question in such terms.

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[1] *Loc. cit.* p. 94.

I took a number of the 'koidoid' films which are made by the Kodak Company to replace the ordinary photographer's plate. Some of these I soaked, without exposure to light, in a solution of sodium hyposulphite until they were cleared, others I exposed for various periods to evenly-diffused dull light, developed them to varying degrees, and then cleared them in the usual way in solution of sodium hyposulphite. In this way I obtained a considerable series of films, each of which transmits a certain fraction of the light thrown upon it.

From this collection of films I chose by the aid of the episkotister and by the method previously described (p. 168) eight films or combinations of films, which transmitted $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, $\frac{1}{6}$, $\frac{1}{8}$, $\frac{1}{14}$ and $\frac{1}{16}$ of the light thrown upon them, respectively.

The film transmitting one-half of the light thrown upon it (which may be designated the $\frac{1}{2}$-film) was then slipped between the edges of the two millboards of which the large disc is made up and secured by a spring-clip in such a position that it filled the gap *W* in the border of the disc. The light from one burner of the lantern transmitted through this film then gives an illuminated circle on the glass plate of brightness equal to $1/8\,L$ of the series of brightnesses of which the action-times had been previously determined (see Table 2, p. 168). The action-time of $1/8\,L$ had been found to be 66 σ, or the time of passage of a point on the disc through 12°, when the disc revolves at 1 revolution per 2 seconds. The gap *W*, containing the film, was therefore made 12° in width, and the gap *N* was made of the same width. On setting the disc in motion at 1 rev. per 2 seconds, the alternate appearances of the circle were of course of unequal brightness, the brighter flash corresponding to the open gap *N*. The width of *N* was then diminished by minimal steps until the difference of brightness between alternate flashes was abolished. This was found to be the case when the width of *N* was reduced to 6°. *N* was then made 3° in width, when of course the flashes given by *N* were very distinctly less bright than those given by *W*. The width of *N* was then increased by minimal steps until the difference of brightness of alternate flashes was again abolished. This was the case when *N* was $5\frac{1}{2}$°.

From this experiment it appeared that light of intensity $1/4L$ excites a sensation of intensity equal to that excited by light of intensity $1/8L$ (*i.e.* of one-half the intensity), acting for its full action-time, when it ($1/4L$) acts upon the retina either for one-half its own action-time or for one-half the action-time of $1/8L$, or for some time intermediate between these two periods. It seemed likely that more accurate work would show the first or second of these possibilities to be true, and that it would prove to be a case of a general law.

In order to render the conditions more favourable to accurate experimental determination of this question, I removed the disc and the screen carrying the glass plate to a distance of 1·5 metres from the lantern and increased a little the rate of rotation of the disc. I then determined, with the greatest possible accuracy on several occasions, the action-time of the circle of light transmitted by the glass plate when at this distance from the lantern and when illuminated by the flame of a single burner. Under these conditions I found the action-time to be represented by a gap of the border of the disc 18° in width. Then placing the $\frac{1}{2}$-film in the path of the ray from the lantern, and so diminishing the intensity of the light by $\frac{1}{2}$, I determined the action-time of light of this intensity and found it to be represented under these conditions by a gap 22° in width.

In a similar manner I determined the action-times of the light when each of the films transmitting $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, $\frac{1}{6}$, $\frac{1}{8}$, $\frac{1}{14}$ and $\frac{1}{16}$ of the light respectively was interposed in the path of the ray from the lantern and so obtained the following figures:—

Action-time of $l = 18^\circ$
,, ,, $1/2l = 22^\circ$
,, ,, $1/3l = 25^\circ$
,, ,, $1/4l = 29^\circ$
,, ,, $1/5l = 33^\circ$
Action-time of $1/6l = 36^\circ$
,, ,, $1/8l = 38^\circ$
,, ,, $1/14l = 42^\circ$
,, ,, $1/16l = 44^\circ$.

Table 3.

It was impossible to determine exactly the action-times of the lower members of this series, but the figures stated are rather too high than too low, for, as will be seen below, a rather too high estimate of these figures introduces no error into the final result, while a too low estimate would have occasioned some error.

The film transmitting $\frac{1}{2}$ the light was then fitted into the gap *W* as before, *W* was made 22° in width, and, the rate of rotation and other conditions being kept constant, that width of *N* (the open gap in the border of the disc) was sought, which on rotation should give flashes of equal brightness with the flashes of *W*. As before, the width of *N* was first diminished by minimal steps until difference of brightness between the alternate flashes was abolished, and then increased from a smaller width until again the difference was abolished.

Then the $\frac{1}{2}$-film was replaced by the $\frac{1}{3}$-film, the gap W was made 25° (the action-time of $1/3l$), and, as before, the width of N was sought at which the difference of brightness of alternate flashes was abolished. This procedure was repeated with each of the eight films, the gap W containing the film being made in each case of the width corresponding to the action-time determined for light of the intensities $1/2l$, $1/3l$, $1/4l$, $1/5l$, $1/6l$, $1/8l$, $1/14l$, or $1/16l$ respectively.

In this way I obtained the figures of the following table :—

Sensation excited by	$1/2l$ in 22° =	Sensation excited by	l in $9\frac{1}{2}°$ ($\frac{1}{2}$ of 18° approx.)
„	$1/3l$ in 25° =	„	l in 6° ($\frac{1}{3}$ of 18°)
„	$1/4l$ in 29° =	„	l in $4\frac{1}{2}°$ ($\frac{1}{4}$ of 18°)
„	$1/5l$ in 33° =	„	l in $3\frac{3}{4}°$ ($\frac{1}{5}$ of 18° approx.)
„	$1/6l$ in 36° =	„	l in $3\frac{1}{2}°$
„	$1/8l$ in 38° =	„	l in 3°
„	$1/14l$ in 42° =	„	l in $1\frac{3}{4}°$
„	$1/16l$ in 44° =	„	l in $1\frac{1}{2}°$.

Table 4.

In this table (as in Table 3 also) the time is in every case expressed in degrees of width of the gap N or W, and, since the rate of rotation of the disc was kept constant throughout, these are all strictly comparable, and it is unnecessary to state the absolute durations of the periods.

The figures of the first column express the action-times found for the light of each of the intensities $1/2l$, $1/3l$ etc.; the figures of the second column express the times during which it was found necessary to allow light of intensity l to act on the retina in order to excite sensations of intensity equal to those excited by the lights $1/2l$, $1/3l$ etc., each acting for a period equal to, or slightly greater than, its action-time. Each of the figures of the second column is the mean between the figures found in the ascending and descending series of minimal changes. The changes were made in steps of $\frac{1}{4}°$ and the figures of the ascending and descending series differed in no case by more than $\frac{1}{2}$°.

If now we consider the upper half of this table only, it appears that the *intensity of the sensation excited by l varies in a manner directly proportional to the duration of its action*, when that duration is from $\frac{1}{2}$ to $\frac{1}{5}$ of its full action-time; or in other words, when l acts for $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$ or $\frac{1}{5}$ of its action-time it excites sensations of the same intensities as those excited by lights of intensities $1/2l$, $1/3l$, $1/4l$ and $1/5l$ respectively acting for their full action-time; that is, the amount of effect (as measured by sensation-intensity) produced by a light acting on the retina obeys the 'photographic law' or varies directly with the duration of action of the light within this limited range.

Turning to the lower half of Table 4 we see an apparent departure from this law, the time required by l to excite sensations equal in intensity to those excited by $1/6l$, $1/8l$, $1/14l$ and $1/16l$ being rather greater than $\frac{1}{6}$, $\frac{1}{8}$, $\frac{1}{14}$ and $\frac{1}{16}$ of its action-time. I am strongly disposed to believe that *the law that the intensity of the sensation varies directly with the duration of the action of the light* of given intensity holds good for all durations less than the action-time of the light, and that the apparent departure from this law shown by the figures of the lower half of Table 4 is due to experimental errors; for in the case of these smaller fractions of the intensity l the determinations are rendered less accurate by two factors;—firstly, the width of the gap N being small, any imperfection of the optical arrangements will occasion a larger error than in the case of the larger fractions, and secondly, the two impressions to be compared in respect of their intensity differ markedly as regards duration in the case of these smaller fractions of l, and this difference renders accurate discrimination of difference of intensity more difficult. This conclusion seems to me to be justified by the fact that we seem to be able to deduce the law above expressed directly from the Talbot-Plateau law.

Talbot himself assumed rather than deduced the truth of this law in his original paper[1]. After describing the 'continuous circle' seen on swinging round a glowing coal, he argued as follows: "The question deserves consideration whether the eye receives from this circular ring exactly the same quantity of light which it received from the much smaller surface of the coal at rest. There can be no doubt, as it seems, that such must be the case; for if the luminous circle sent more rays to the eye, it would likewise send more in every other direction, and thus the apartment would become more illuminated than before, which is not the fact. If, then, the total quantity of light remains the same, it follows that its apparent intensity must have diminished exactly in the same proportion as its apparent area has been enlarged. For the sake of more accuracy, if we confine our reasoning to a very small portion of the luminous body, the enlarged area which it seems to occupy is evidently proportional to the circumference of the circle it describes; therefore the preceding argument alleges that the intensity of light diminishes as the circumference increases. But in the same proportion as the circumference increases, the time during which the coal is found in any particular point of the circle diminishes." "Since, then, these two things—the intensity of the light and the time of the body's remaining in any given part of the circle—are each inversely proportional to the circumference of the circle it describes, it follows that they must be directly proportional to each other; that is to say, that a regularly intermittent luminary, whose observations are too frequent and too transitory for the eye to perceive, loses so much of its apparent brightness from this cause, as is indicated by the proportion between the whole time of observation and the time during which it disappears."

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[1] *London, Edinburgh, and Dublin Philos. Mag.* 1834, Ser. 3, vol. v. p. 327.

It must, I think, be admitted that the law expressed above is obscurely indicated in this curious passage; but it was deduced with perfect clearness by W. Swan in the paper before quoted[1].

Working apparently in ignorance of the earlier work of Talbot and Plateau, he showed that, when a disc of closed and open sectors is rotated before an illuminated surface at rate high enough to give fusion of the successive impressions, (1) increase of rate of rotation produces no alteration of the apparent brightness, and (2) that a disc with one open sector of 20° gives the same apparent brightness as one with two open sectors, each of 10° only, rotated at the same rate, as well as other of the facts resumed by the Talbot-Plateau law; and he deduced with perfect propriety, as it seems to me, the following law, "When light of a given intensity acts on the eye for a short space of time, the apparent brightness of the luminous impression on the retina[2] is exactly proportional to the time during which the light continues to act" (p. 504). This deduction he justifies by the following reasoning: "It has been proved that the brightness of the impression produced by rapidly-succeeding flashes of light is proportional to the number of flashes in a given time, provided the brightness of the flashes remains constant. Hence, if a rapidly-revolving disc, with a sector [open] of a given angle, has its velocity doubled, and, consequently, the number of flashes produced by it in a given time also doubled, if the brightness of the flashes remains unaltered, the brightness of the impression produced by it will be twice as great as at first. But, instead of the brightness of the impression increasing, it has been found to continue unchanged, notwithstanding the increased velocity. It is, therefore, evident that when the velocity of the disc is doubled, and, consequently, the duration of each flash is half as great as at first, its brightness is also half as great as at first. Thus if the disc first revolves 20 times in a second, and then 40 times in a second, the intensity of the impression is precisely the same in both cases. But at the velocity of 40 revolutions in a second there are twice as many flashes in a given time as there are at the first velocity; and if the brightness of the flashes was the same as at the first velocity, the brightness of the impression produced by them would be doubled. Since, therefore, the impression, instead of being doubly bright, remains the same as at first, each flash at 40 revolutions in a second must be only half as bright as at 20 revolutions in a second."

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[1] *Trans. Roy. Soc. of Edinburgh*, 1849.

[2] This phrase evidently means what is commonly called the intensity of the sensation.

Many authors have discussed the interpretation of the Talbot-Plateau law in recent years, but the perfectly natural and satisfactory explanation suggested by Swan in the above passages has not, so far as I know, been accepted by any of them, and, of course, so long as black following white is regarded as a stimulus to some form of positive physiological process, this explanation cannot be accepted. I think that the experimental proof adduced in this paper that the interpretation of the law suggested by Swan is true throughout a certain range of durations renders it in the highest degree probable that that interpretation is true for all durations, and incidentally provides further evidence, if that be needed, that the assumption of a black-exciting process is superfluous.

The law deduced by Swan finds further experimental support in the law of Bloch[1], experimentally established by him and confirmed by several other observers, notably M. Charpentier[2]. This law states that in the case of a light of brief duration and of such intensity as to be only just perceptible, the intensity must be varied inversely as the duration in order that the light may remain just perceptible, *e.g.* if a light of a certain intensity is just perceptible when its duration is ·02 sec., then if its duration is reduced to ·01 sec. its intensity must be doubled in order that it may remain perceptible. That is to say, that in this case of the just perceptible light the intensity of the effect produced varies inversely as the duration. The case of the just perceptible light is but a special limiting case, and the fact that this law is found to hold in this case lends a certain support to the conclusion that it holds in the case of lights of all intensities.

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[1] *C. R. de la Société de Biologie*, 1885, huitième Série, t. 2, p. 495.

[2] *Ibid.* 1887, t. 2, p. 5.

Here may be added an account of an experiment made with the object of determining roughly the rate of decline of the intensity of the sensation as the time of action of the light is prolonged beyond the action-time.

The apparatus being arranged as in the series of experiments described in § 2, and the disc rotating at 1 revolution per 3 seconds, the action-time of a white light of moderate brightness was found to be $66\,\sigma$; *i.e.* the difference of intensity of flashes *N* and *W* was abolished when the open sectors *N* and *W* were made 8° and 12° in width respectively. The open sector *W* was then increased to 30°. On rotation the flash *N* then appeared very distinctly brighter than *W*. The width of *N* was then reduced by minimal steps until the two flashes once more appeared to be of equal brightness. This was found to be the case when *N* was 5° in width.

Now, it has been shown above that when such a light acts for a period equal to one-half its action-time only it excites a sensation of the same intensity as a light of one-half the intensity acting for its full action-time, and it may be assumed that when it acts for $\frac{5}{8}$ of its action-time the intensity of the sensation is reduced in the same proportion. It would seem then that when the action of the light is prolonged about $184\,\sigma$[1] beyond the limit of its action-time the intensity of the impression declines by as much as $\frac{3}{8}$ of its whole intensity, or in round numbers it may be said that when the action of light of this particular brightness is prolonged about $\frac{1}{5}$ second beyond the limit of its action-time, the intensity of the impression declines about 50 %, *i.e.* the impression produced appears equally bright with one produced by a light of half the physical intensity acting for a period equal to its action-time only.

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[1] $184\,\sigma$ is the difference between $66\,\sigma$ the action-time of the light and $250\,\sigma$ the duration of its action when *W* is made 30° wide.

The rate of decline is thus astonishingly rapid, a fact previously demonstrated by M. Charpentier in the case of a bright area moving across the field of view of the resting eye. The bearing of this fact upon the conclusions of Prof. Götz Martius has already been pointed out (p. 171).

§ 5. *The action-time of light affecting the rods of the retina only, and of the just perceptible light.*

I next attempted to determine the action-time of light when the sensation is excited through the rods of the retina and the retino-cerebral apparatus for vision in a dim light. For this purpose I interposed a plate of green glass in the path of the ray from the lantern and arranged a small luminous point to serve as fixation-mark about 7 cm. from the circle of green light. The illumination of the circle was then diminished until, to the dark-adapted eye directed to the fixation-mark, it appeared quite colourless. I then proceeded as before to compare the intensity of alternate flashes. Since it was difficult to appreciate small differences of brightness of images falling on the peripheral retina the durations of *N* and *W* were kept in the proportion of about 2—3, and the rate of rotation of the disc was reduced to 1 revolution in 3 seconds. The shorter flash, *N*, was then distinctly duller when the two gaps *N* and *W* were 20° and 30° respectively and the difference was not certainly abolished until *N* and *W* reached the width 27° and 40° respectively. But owing to the impossibility of discriminating small differences of intensity in peripheral vision the figure 27° (which equals 225 σ) cannot be regarded as an exact determination, and the experiments cannot be held to do more than establish the fact that the action-time of light, acting upon the rods only, is considerably greater than 166 σ (= 20° at 1 revolution per 3 seconds) and that it is probably slightly greater than 200 σ or $\frac{1}{5}$ second.

When this point of my research had been reached it was suggested to me by my brother, who had kindly allowed himself to be put through a part of the series of experiments described above and whose determinations agreed closely with my own, that these determinations might have a bearing upon the principles regulating the flashing lights of our modern lighthouses. These principles must be very largely dependent upon physiological and psychological facts, and therefore I make no apology for presenting the matter of this section in the pages of a psychological journal.

On turning to works upon the principles of lighthouse construction I found that all the engineers seem to accept $\frac{1}{10}$ second, or slightly more or less, as the action-time of light of all intensities. Thus Mr W. T. Douglas, in his article 'Lighthouse' in the supplementary volumes to the 9th edition of the *Encyclopædia Britannica*, writes, "Duration of flash necessary for physiological perception:—The researches of numerous physiologists and physicists, among whom are Plateau, Helmholtz, Wundt, Bloch, and more especially Charpentier, confirm the experimental results obtained by M. Blondel at the Dépôt des Phares in Paris, that a flash of $\frac{1}{10}$ second is sufficient for the purpose of impressing a ray of light upon the retina of the eye. His researches proved that the exact time of full perception varies from ·08 to ·125 second. The French Lighthouse-Service accordingly fixed the standard minimum duration of flash at $\frac{1}{10}$ second. This length of flash thus represents the standard of maximum efficiency of a light, and any increase in the duration naturally results, *ceteris paribus*, in a corresponding diminution in the intensity."

Baron de Rochemont (Directeur du Service des Phares) in a paper read before the International Engineering Congress at Glasgow in 1901 writes, "Experience has shown that there is no advantage in prolonging the duration of the flash beyond $\frac{1}{10}$ second, which is about the minimum time required for full perception."

At the same congress Mr A. Brebner[1] read a paper in which he stated, "The fact that a light produces its full effect on the eye in one-tenth of a second, was experimentally determined by Mr Swan in 1849."

The only report of M. Blondel's experiments that I have been able to discover is very brief[2]; but from this report it would appear that he has not himself attempted to determine the action-time or, as he calls it, the "time necessary for full perception." He points out that "the minimum perceptible light, by definition, is not seen unless it has time to produce its full impression. The time of appearance of this light is then identical with its time of full perception." Of this time he states, "It is at present very badly determined."—"It would seem to be in the neighbourhood of $\frac{1}{10}$ second, and this figure is confirmed by two experiments of M. Charpentier, which have given him the values 0·08 second and 0·125 second." Now the experiments of Exner (accepted by Helmholtz) and of Martius, and those described in the first section of this paper, agree in showing that the action-time increases markedly with diminution of intensity of the light, and that the action-time of very dim light is much greater than $\frac{1}{10}$ second. Now I take it that the light to which the lighthouse-engineer must seek to give the most favourable duration is one so dim that it can only just be perceived by the peripheral retina of the dark-adapted eye of the mariner. I do not know whether sailors generally are aware that they pick up and observe distant lights, that are only just visible, by the use of the peripheral retina. Since astronomers seem to have remained ignorant of the superior sensitivity of the peripheral retina, as compared with the rodless fovea, for some thousands of years, it seems likely that sailors are not commonly aware of this distinction and do not deliberately apply this knowledge. But that they do thus make use of the peripheral retina seems to be quite certain, for to the dark-adapted eye of the man on the look-out at the bow of a vessel a light that is very distinctly perceptible in peripheral vision must be quite invisible when its image falls on the fovea. Even then, if the experiments hitherto made on the action-time of light and relied on by the engineers were otherwise reliable, they would be quite inconclusive as regards their problem so long as the observations were made by the use of the rodless fovea and were not made exclusively by the peripheral retina and with the use of light so dim as to affect the rods only of the dark-adapted eye. I believe that none of the experiments made hitherto have taken account of these considerations.

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[1] "Improved rapid group-flashing lights."

[2] "On Flash-lights and the physiological perception of Instantaneous Flashes," *Proc. of International Maritime Congress, London*, 1893, Sect. IV. p. 89.

What is needed is, then, a series of experiments which shall determine the action-time of a light presented to the peripheral retina and so dim as to be only just visible when its duration equals or exceeds its action-time, or in other words the problem is—Given a light so dim as to be only just perceptible to the dark-adapted eye in peripheral vision, how long must it act upon the retina in order to be visible?

In order to answer this question I proceeded as follows:—The apparatus was arranged as in the last experiments (p. 181), the rate of rotation of the disc was left at 1 revolution per 3 seconds, and the iris-diaphragm was shut down until the circle of light was only 2 mm. in diameter. The illumination was then cut down until, when the thoroughly dark-adapted eye at a distance of 50 cm. was directed to the small and dim fixation-point or its immediate neighbourhood, the circle 2 mm. in diameter was only just perceptible when continuously illuminated. The two open sectors or gaps of the disc, *N* and *W*, were then made 10° and 45° in width respectively. On rotation of the disc at 1 revolution per 3 seconds the circle then became visible at intervals of 3 seconds, at moments corresponding to the passage of *W*; *i.e.* the briefer illumination through *N* was insufficient to render the circle perceptible. The width of *N* was then increased by small steps, the observations being repeated after each step, until the circle became visible on the passage of *N* as well as on the passage of *W*. It then appeared that not all points in any one small area of the peripheral retina are equally sensitive, *i.e.* have equally low thresholds of sensitivity[1]. When *N* was 18° in width (= duration of 150 σ) the flash *N* was sometimes visible, sometimes invisible, as the fixation of the eye changed slightly, either voluntarily or involuntarily. And it was not visible at every passage of *N* until *N* was made 22° in width. This difference of sensitivity of neighbouring points of the retina complicated the experiment and rendered it incapable of yielding an exact determination of the duration sought. And it was therefore necessary to adopt a more refined procedure and one which should be entirely without knowledge of the nature of the experiment on the part of the observer.

Having secured the services of an attentive subject I replaced the gas-light of the lantern by a small steadily burning oil-lamp, the flame of which was adjusted until the circle 2 mm. in diameter continuously illuminated was just distinctly perceptible to the subject as he sat with the dark-adapted eye 50 cm. from the circle and directed towards a small fixation-mark 5 cm. distant from the circle. The open sectors *N* and *W* were made 10° and 35° wide respectively and the disc set in rotation at 1 revolution per 3 seconds. The subject was instructed to say 'now' every time he perceived the faintest appearance of light in the position of the small circle. He saw the flash at every passage of *W*, but did not perceive any flash at passage of *N*. The width of *N* was then increased by steps of 2° each and the subject perceived no trace of the flash *N* until *N* was 20° in width. While the flash *W* was then seen at every passage of *W*, the flash *N* was seen occasionally only.

The intensity of the illumination was then slightly reduced by putting a clear film in the path of the ray in the lantern. Flash *W* was still seen at every revolution, *N* was not seen at all. Under such conditions most persons are apt to observe slight flecks of light in the dark field that have no objective source but may easily be mistaken for the objective flash. The ray of light was therefore completely interrupted for a few seconds at frequent intervals throughout the experiments while the subject looked out for and reported the appearances of the flash. In this way errors due to such subjective flecks of light would have been detected; but it was found that they did not occur.

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[1] Owing, no doubt, to the presence of cones among the more numerous rods.

N was then increased to 22° and the flash *N* was then seen at rare intervals, while *W* was seen at every passage of *W* with rare exceptions. The light was then again reduced very slightly and *N* was made 24° in width. The flash *W* was then perceived at almost every passage of *W*, remaining unnoticed about once in every five or six flashes, while *N* was seen almost but not quite as frequently as *W*.

When *N* was made 26° in width the difference between the constancy of perception of *N* and *W* was abolished; 24°, or very slightly more, represents, then, the duration sought, and as the disc rotates at 1 revolution per 3 seconds, 24° represents a duration of 200 σ or $\frac{1}{5}$ second.

A similar series of experiments made on a second subject gave exactly similar results.

Working alone I have repeated these observations on many occasions with variations of the details of the experiments and have constantly found the same result, *i.e.* that the dimmest light visible to the peripheral retina of the dark-adapted eye (*i.e.* the dimmest light perceptible under the most favourable conditions) must be allowed to act for a period not less than 200 σ or $\frac{1}{5}$ second in order to be perceptible. The 'time of appearance' or action-time of the minimum perceptible light would seem then to be about twice as great as has usually been assumed, namely $\frac{1}{5}$ second rather than $\frac{1}{10}$ second. The question whether this fact has any bearing upon the principles of flash-light construction must be left to the decision of the engineers[1].

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[1] M. Blondel argues in the paper quoted above, that an exact knowledge of the length of this period is of no practical importance because all that is necessary is that the duration of the flash shall be not greater than this period; for according to the law of Bloch, for the production of the minimal impression "a certain quantity of light suffices, which is independent of the intensity and the form of the flash, provided that it is concentrated in a time of illumination less than the maximum time." It would seem then that if the law of Bloch is strictly true the only practical importance of the above determination of the action-time of light of the least perceptible intensity is to show that the duration of the flash may be made as much as $\frac{1}{5}$ second without loss of effective luminosity, and that the $\frac{1}{10}$ second, generally accepted as the upper limit of permissible duration, is needlessly small.

§ 6. *The action-times of rays of different refrangibilities.*

In 1874 A. Kunkel[1], after failing to obtain any results by the use of Exner's method, adopted a different procedure with the same apparatus and applied it to the determination of the action-times of lights of different colours. The procedure was as follows. Two small fields of light from the same region of the spectrum were presented to the eye by the eye-piece of the telescope, a duller one *a* lying immediately above a much brighter one *α*. The field *a* was illuminated a small fraction of a second before the field *α*. The assumed course of the two processes excited by the two fields is represented by two curves of the form shown in the figure 4 (modified from Kunkel's figure).

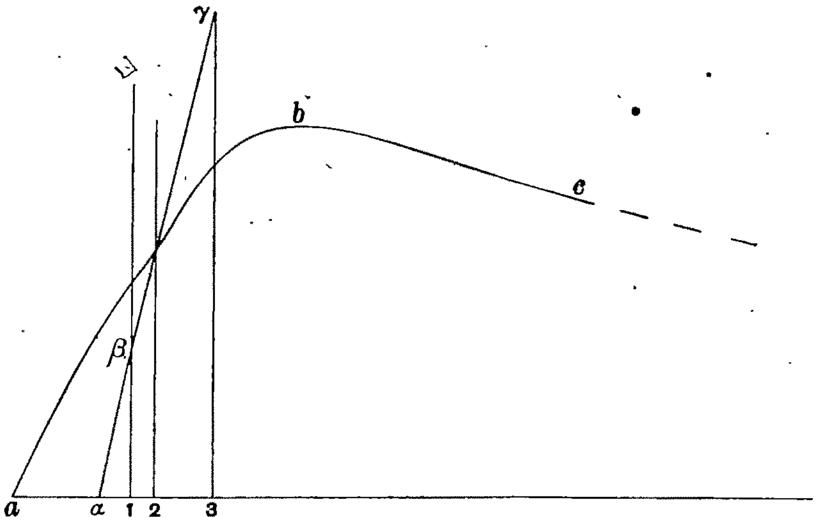

FIG. 4.

The curve *abc* rises slowly to a maximum and then at once begins slowly to fall, the curve *αβγ* rises very steeply and rapidly and must soon overtake the curve *abc* and rise above it. If then both fields are obscured at the moment represented by the vertical line 1, *a* should appear the brighter, if at moment 3, *α* should appear brighter, and if at moment 2 they should appear equally bright. The method adopted was to take a series of durations of *a* and at each of these to find out by trial what duration of α was necessary to make it appear of equal brightness with a. As the duration of a was increased step by step, it was found that up to a certain point this duration of α increased also, but diminished beyond that point. The duration of a which required the longest duration of α to give equality of brightness of the two fields was then accepted as the measure of the duration of action of a necessary for development of its full effect, *i.e.* as the measure of the action-time of a.

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[1] "Ueber die Abhängigkeit der Farbenempfindung von der Zeit." *Pflüger's Arch.* 1874, Bd. IX. S. 197.

By means of this procedure Kunkel arrived at the conclusion that red, green, and blue lights of about the same brightness have action-times equal to ·0573 sec., ·133 sec., and ·0916 sec. respectively.

Now if this conclusion must be accepted it constitutes a very serious difficulty for Young's theory of the colour-processes; for if this theory be true and Kunkel's conclusion also be true, a white light exposed to the eye for only ·05 sec. or less must appear always red, and this is not the case[1].

Kunkel's conclusion must, then, be carefully examined. In the first place we may note that his procedure is subject to most of the objections taken above (p. 155) to the procedure by which Exner sought to determine the action-time of white light, namely the incalculable effects of simultaneous contrast, and the difficulty of discriminating difference of brightness of two fields presented simultaneously and momentarily only. But these deficiencies of the method, though they suffice to render doubtful the absolute values of the times found, do not explain away the fact that green light was found to require nearly three times as long, and blue light nearly twice as long, as red light to produce their full effects in sensation. My own experiments, presently to be described, seem to prove that the action-times of red, green, and blue lights of the same intensity differ but little or not at all.

Kunkel's anomalous results are, I believe, completely explained by the fact that the rod-apparatus contributed a considerable part of the total brightness in the case of the green and blue lights, and none in the case of the red light to which the rods are insensitive. For it is highly probable that the images of the two adjacent fields of Kunkel's experiments were not confined to the rodless fovea, and it is clear that the spectral light of which the action-time was to be determined was of low intensity, for the source of the light was an oil-lamp and the light was admitted through a very narrow slit; and it is clear also that the eye of the observer must have been, under the conditions of the experiment, moderately dark-adapted.

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[1] The red borders of briefly exposed white areas, described by Mr S. Bidwell, do not constitute an exception to this statement.

Now it has been shown above (p. 181) that the action-time of light exciting the rods only is large, approximately ⅕ second; but more important is the fact demonstrated in a previous paper[1] that the sensation excited through the rod-apparatus comes into being more slowly than that excited by the feeblest stimulation of the cones, the difference amounting to about 55 σ. It is clear that in the case of green light of low intensity, falling upon the extra-foveal region of the dark-adapted eye, the curve representing the intensity of the sensation (*a* in figure 4), must be a composite curve representing the effects produced through both rod- and cone-apparatus; and since the curve representing the effects of excitation of the rod-apparatus reaches its maximum 55 σ later than that representing the intensity of the colour-component the former will be superimposed upon the crest of the latter so that the composite curve, instead of beginning to fall from the maximum point of the curve of the colour-component, will continue to rise for about 55 σ longer. If then the curve representing the green-exciting process and that representing the red-exciting process are identical in form and reach their highest points in the same number of units of time, the curve representing the composite process excited by the green light (in both rod- and cone-apparatus) will continue to rise, and will reach its maximum some 55 σ later than the curve representing the simple colour-process excited by the red light. The time elapsing between the beginning of the stimulation and the rise of the curve to its highest point was the time measured by Kunkel, and therefore, unless the images of the two fields were confined to the rodless fovea, the time found for green light must have exceeded that found for red light by about 55 σ, even though the true action-time of red and green lights, acting upon cones only, be identical. And 55 σ is but little less than the difference found by Kunkel, namely 75 σ.

Similar considerations, together with the fact that blue light acts less intensely on the rods than does green light, suffice to account for the smaller difference, amounting to 34 σ, found by Kunkel between the action-times of red and blue lights.

I determined the action-times of red, green, and blue lights by experiments similar in every respect to those by which the action-time of white light had been determined, and though the determinations are more difficult in the case of coloured light, owing to the complicating effects of differences of saturation accompanying differences of intensity, the figures found are, I think, but little less accurate than those found in the case of white light. The light was rendered approximately homochromatic in each case by filtering it through several layers of carefully selected coloured gelatines. The red, green, and blue gelatine filters were so adjusted as to give red, green, and blue images of equal brightness, as judged by the eye. In all cases the differences of the action-times found were such as might well be due to unappreciated differences of brightness of the differently coloured ima[illegible] Thus in one series I found the following action-times,

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[1] This *Journal*, No. 1, p. 108.

	Red	Green	Blue
	100 σ	106 σ	108 σ;
in another series of lower intensities,			
	133 σ	133 σ	[illegible]2 σ;
in another,	112 σ	125 σ;	
and again,	125 σ	125 σ.	

In these experiments any error that might arise from the mixture of rod-excitation was avoided, because the image (7 mm. in diameter at distance 50 cm. from the eye) fell wholly on the rod-free fovea. This result is to some extent supported as against Kunkel's by the fact that Martius[1] found the action-times of red, green, and blue lights to be in the inverse order to that found by Kunkel, namely, 90 σ, 48 σ, and 36 σ respectively. Allowing for the fact, stated by Martius, that the red was the least bright of the three coloured lights used in his experiments, these figures bear out the conclusion indicated by my experiments, namely, that there is no considerable difference between the action-times of red, green, and blue lights when these are of equal intensities, *i.e.* of such intensities as to excite sensations that appear to be of equal brightness.

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[1] *Op. cit.* S. 335.

PROCEEDINGS OF THE PSYCHOLOGICAL SOCIETY.

Jan. 30, 1904. Subconsciousness, by JAMES WARD.
The Colour-names of Young Children, by W. H. WINCH.

Mar. 12, 1904. Four Cases of Cerebral Lesion or Defect in reference to Speech Localization, by F. W. MOTT.
The Psycho-genetic Theory of Comparison, by F. N. HALES.

The material for such a theory is to be found in the indirect evidence afforded by linguistic expressions of judgments of comparison. In the absence of any reliable data as to the development of comparative speech-forms in the individual, an appeal must be made to comparative philology in its widest sense. A review has accordingly been made of the various methods used by the different families of spoken langua[illegible]d of the gesture languages of primitive peoples and of the deaf-and-dumb[illegible]

Spoken Languages.—The main principles underlying expressions of comparison-judgments are (i) Opposition; a quality is affirmed of one object and denied of another: (ii) Separation; a postposition, preposition, adverb, verb or case-ending is used, implying the concept of 'motion away from,' the adjective itself remaining unchanged: (iii) Gradation; the object is modified by some affix, meaning 'more,' 'very,' etc.: (iv) Composition; a paratactic conjunction is used. There is a continuous philological development from each principle to the next. Separation co-exists with Gradation and survives among the most modern instances of Composition.

Gesture Language.—There are four stages of development. The earliest corresponds to Opposition. The second corresponds to the occasional use in spoken languages of different words to express different degrees of the same quality. In the third stage the principle of Separation is clearly developed, and in the fourth the principle of Gradation.

It is claimed that these linguistic uniformities require, and are capable of, explanation in the light of analytic theory; that they can be accounted for by the so-called Absolute Impression Theory of the comparison-process; and that they afford additional proof of the truth of that theory.

VOLUME I JANUARY, 1905 PART 3

MALEBRANCHE'S THEORY OF THE PERCEPTION OF DISTANCE AND MAGNITUDE.

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Malebranche's account of the signs of distance. His analysis of the perception of magnitude. His controversy with Regis. His theory of natural judgment, and his account of the contrast between sense-perception and science. His occasionalistic theory of perception. Criticism of Malebranche's theory. His view of natural judgment leads to the conclusion that all perception is purely sensational.

MALEBRANCHE'S theory of the perception of distance and magnitude, which forms an interesting anticipation of Berkeley's epoch-making work, has not received the attention it deserves. It is not even mentioned by Ollé-Laprune, Malebranche's chief commentator. Yet the theory is developed at considerable length both in the *Recherche de la Vérité* and in the *Réponse à M. Regis,* and illustrates, perhaps better than any other part of his teaching, his highly individual psychological position. As the theory has not only historical, but also great intrinsic interest, I propose in this article to give an account of it, showing its place in Malebranche's philosophy, and comparing it with that of Berkeley.

I shall start with the account given in the *Recherche*[1]. Recognising that as distance is a line directed endwise to the eye it cannot of itself, and immediately, be seen[2], Malebranche sets himself to examine the various signs of which we make use in estimating it. He seeks to prove that as they are all without exception uncertain, the judgments which we base upon them must remain fallible, and can only be approximate. In his account of what he regards as the most universal, and in certain cases the surest sign of distance, viz. the angle formed by the optic axes meeting at the object, Malebranche at first sight appears to be entirely unconscious of the difficulties involved. "When this angle is very large, we see the object very near, and on the contrary when it is very small, we see it as very distant. And the change which takes place in the situation of our eyes according to the changes in this angle, is the means of which our mind makes use in judging of the distance or proximity of the objects[1]." It is because this sign applies only to near objects that "we see the sun and moon as if they were enveloped in the clouds, although they are extremely distant from them; that we naturally believe the stars to be all at an equal distance, and the comets to be stable and almost without motion towards the end of their course[2]." Malebranche goes on to speak of the act of judgment whereby we estimate the distance of the object as "a kind of natural geometry," and so seems to lay himself open to all the unanswerable objections by which Berkeley has refuted such teaching. Malebranche adds, however, that these 'natural judgments' are sensations, *sensations composées*[3]. Thereby he separates himself off from the ordinary crowd of Berkeley's opponents; and his theory as to the connexion between the signs of distance and the judgments which are based upon them, may, as I shall try to show, challenge comparison with Berkeley's own explanation through association. This, however, is the most important and difficult point in the whole problem; and I defer consideration of it until I have completed Malebranche's account of the remaining signs of distance and magnitude. With regard to them there is not the same ambiguity of statement.

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[1] Liv. I. chap. IX.

[2] This, which has sometimes been spoken of as Berkeley's great discovery, was recognised, as Berkeley himself points out, by all contemporary writers on optics. Cf. *Essay towards a New Theory of Vision,* § 2. The diagram given by Malebranche to illustrate the possible illusions to which our apprehension of distance and movement is subject, rests on this principle.

The second sign of distance consists in the muscular sensations accompanying the focusing of objects. This sign, likewise, only applies to near objects. If an object is half a foot from us, we can distinguish the distance by means of the muscular sensations which are painful. If the object is 2 feet distant, the muscular sensations are still appreciable, though not painful. But if the object is removed several feet, the sensations are no longer experienced, and this sign of distance ceases to be available. The third sign consists in the magnitude of the retinal image. It diminishes with the distance of the object represented, though less and less appreciably as the distance increases. This sign obviously involves previous experience. We must have learned from experience what is the actual size of the objects, in order from their apparent size to determine their distance: we do not see the larger stars as nearer than the smaller, but all as at the same distance. Fourthly, we estimate distance by the force, distinctness, and definiteness of the retinal image. "Objects which are not brightly illumined or that we see confusedly appear more distant than they really are, while, on the other hand, bodies that are luminous and are seen distinctly, appear much nearer[1]." The fifth, and last, sign is the number and kind of the intervening objects. This is the most important of all the signs of distance. "The confused view of the lands which are between us and a tower is the same thing as the natural judgment of the distance of the tower, for those judgments are nothing but compound sensations[2]."

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[1] *Loc. cit.* § 8. [2] *Loc. cit.*

[3] Cf. liv. I. chap. VII. § 4, "Comme les sens ne font que sentir et ne jugent jamais à proprement parler, il est certain que ce jugement n'est qu'une sensation composée."

Still deferring the problem, to which the last sentence brings us back, as to the nature of the judgment by which we estimate distance, I shall further prepare the way for its adequate treatment, by next considering Malebranche's analysis of the perception of magnitude. That magnitude, even *apparent* magnitude, is estimated or inferred, and therefore stands on exactly the same level as distance, Malebranche saw very clearly; and it is with reference to the perception of magnitude that his most interesting statements are made. Thus in the *Recherche*, at the end of the chapter from which I have been quoting, and in connexion with the last-named sign of distance, he contends that a tower viewed as standing beyond intervening fields and houses is seen as larger than it would appear if artificially isolated and viewed alone. He illustrates his contention by applying it in explanation of the varying size of the moon at the horizon and in midheavens. When we see the whole landscape between us and it, we *judge* it to be very distant, and so *perceive* it as larger. When, on the other hand, it is in midheavens, we have only the height of a house as our standard: there is not the same sensible compulsion laid upon the mind to refer it out in space, and it is therefore seen as smaller.

This solution, that a mental estimate of distance should affect actual *perception* of size, was challenged by Regis in his *Système de*

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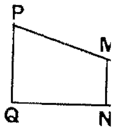
[1] Liv. I. chap. IX. § 3.

[2] *Loc. cit.*

Philosophie. He argues that as an astronomer's know actual distance and size of the moon has not the least perception of its apparent size, the cause assigned is there be inadequate. His own view is that apparent magnit solely upon the size of the retinal image, and that the inc the retinal impression of the moon at the horizon is due of its rays by vapours in the air.

These objections gave rise to Malebranche's mos *Réponse,* in which he develops his theory with remarkable showing complete appreciation of the important, and consequences that follow from it. The difference betwee and the apparent magnitude of the moon at the horizon[1] but one instance of the way in which all visual sensa fluenced by our judgments of distance. Only by assumi are thus transformed, can we account for the apparent ma of objects in our immediate neighbourhood.

He constructs the following diagram. The line *QA* is be bisected at *N*. "Though the object *PQ* is double the object *MN*, and is twice as distant from the eye *A*, yet since it is seen under the same angle, it must trace on the retina an image equal in size to that traced by *MN*.... Now, when we notice the distance, the height of the object *PQ* appears about double that of the object *about* double, because we cannot by sight estimate exactly of objects. A dwarf, two paces from us, certainly appears r than a giant, three times as large, who is distant six pa both are seen under equal angles, or what is the sam though the images which are traced on the fundus of the e As this inequality in the apparent magnitudes does not a inequality of the visual angles, or of the images which equal in the fundi of our eyes, it must be due to the the distance[2]."



That this is the true explanation of the variations in magnitude of the sun and moon Malebranche proves ex He asks us to take a flat piece of glass and so darken it of a candle-flame, that looking through it we can distinctly see the flame of the candle but cannot see any non-luminous objects. Through such a glass, when held close to the eyes, so that it completely eclipses the earth and sky, the sun and moon appear of equal size at the horizon and in the meridian. "If the sun is at the horizon the interposition of the glass will make it appear about twice as near, and about four times as small; for here precision is not necessary. But if it is high above the horizon, the glass will produce no considerable change either in its distance or in the apparent size[1]." Since the interposition of the glass does not alter the size of the images traced on the retina, and yet diminishes the apparent size of the sun and moon, the normal variation in their size must be due to the sensible apprehension of the intervening landscape[2].

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[1] The retinal image of the moon at the horizon, so far from being larg and others had shown, slightly smaller, the moon being then more dist diameter of the earth.

[2] *Réponse à M. Regis,* I. §§ 1—3.

In reply to Regis' argument that an astronomer's knowledge of distance exercises no influence upon his apprehension of the apparent size of the heavenly bodies, Malebranche emphasises the important point that distance, in order to have any effect on our perceptions of magnitude, must be actually perceived by the senses. "A distance that is not actually perceived by the senses must be counted as nothing, and cannot serve as a basis for the natural judgment which is formed in us of the magnitude of objects[3]." Malebranche's explanation of this difference, fundamental as regards its effects, between sensible and merely intellectual knowledge, I shall consider shortly.

To Regis' further contention that if the judgment, that objects are distant, influenced their magnitude at all, it would render them smaller, not larger, in size, Malebranche replies by a very convincing illustration. "Are we to believe that if M. Regis viewed a landscape from the middle of his chamber, all that he discovered there would, by his fundamental law that the apparent size of objects depends solely on the magnitude of the images which they trace on the retina, appear smaller than his window, and that a mountain, for instance, must appear smaller than the window, since its retinal image is contained in that of the window? For if, in order to conclude that the mountain is very great, he should judge that it is very distant, this judgment would make it appear smaller, according to his optical principle[1]."

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[1] *Loc. cit.* § 6.

[2] Berkeley's account of visual magnitude is not so satisfactory as that of Malebranche. His sharp distinction between the visual and the tactual causes him to misrepresent the actual facts. Cf. *Essay on Vision*, § 60: "A man placed at 10-foot distance is thought as great as if he were placed at the distance of 5-foot; which is true, not with relation to the visible, but tangible greatness of the object: *the visible magnitude being far greater at one station than it is at the other*." Berkeley did not realise to the same degree as Malebranche the relative, inferential character of visual magnitude. Malebranche, on the other hand, gives no adequate account of the important part which touch plays in the visual apprehension of magnitude; and his theory therefore requires to be supplemented by that of Berkeley.

[3] *Loc. cit.* § 4.

We can now turn to Malebranche's theory as to the relation in which the signs of distance and magnitude stand to the judgments which are based upon them. Judgments, according to Malebranche, are involved in every act of sense-perception: through them alone our sensations are enabled to fulfil their practical function in the mental life[2]. Thus while the only objects we can immediately apprehend are ideas in the mind, through natural involuntary judgments we interpret them as real external existences. And of exactly the same nature are all those judgments through which the muscular and other sensations of sight become the apprehension of distance and magnitude. These judgments are not executed, and cannot be controlled, by the finite mind: they are involuntary, irresistible. Try as we may, we cannot apprehend colour as a purely subjective affection of the mind. An enlightened philosophy may prove that it is such, but we must none the less continue to envisage it as the quality of an external object. In a similar manner, we may be convinced that the moon is no larger at the horizon than in midheavens, but such belief exercises not the least influence on our perception. These judgments are false, and yet irresistible. Their falsity may be detected, but still they persist. Usually each such natural judgment is accompanied by belief in its truth, that is to say, by a voluntary judgment whereby the natural judgment is confirmed[3]. But, as the frequent conflict between philosophical truth and sense-experience proves, the voluntary and the involuntary judgments are separate independent acts of mind.

Malebranche's general principle is, therefore, that every sense-perception involves natural judgments, many of which are false[4]. And he is keenly interested in establishing this principle, since it enables him to illustrate in a striking manner his favourite distinction between sense-perception and science. To identify sense-experience and science is to misrepresent both. The aim of science is to discover eternal truths about real existence, and its data are the clear ideas of pure understanding. Such knowledge is consciously constructed by the individual mind. Sense-experience, on the other hand, is concerned only with the knowledge that is necessary for the practical purposes of life. It is constructed in the light of the effects which objects produce in the body, and involves at every moment renewed adaptation to a multitude of constantly changing states. With every movement of the head the field of vision changes, and in its apprehension a thousand different judgments, each of which rests upon complicated data, are instantaneously formed. To explain such experience Malebranche therefore insists—and this is perhaps the most characteristic feature of his theory—upon the supra-individual nature of all our perceptions. In their construction the finite mind can take no part.

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[1] *Loc. cit.* § 11.

[2] *Recherche*, liv. I. chap. x. § 6.

[3] *Loc. cit.*

[4] *Recherche*, liv. I. chap. xiv. at the beginning.

When Malebranche is dealing with the judgments whereby subjective sensations are interpreted as qualities of external objects, he sometimes speaks of them as if they were instincts or habits. They are invariable, being brought into action by every given sensation. In analysing, however, the processes involved in the perception of distance and magnitude, he evidently came to realise more and more clearly the complexity and variability of the natural judgments, and consequently offers of them a very different explanation. He tells us that he names them *natural* judgments, in order specially to mark that they are not framed by the finite mind. All the innumerable, varied effects which a landscape produces in the eyes and brain, form the data of these judgments, and they are, as we find on examination, interpreted with perfect accuracy, in accordance with true optical principles, and with marvellous rapidity. Obviously, then, such judgments are as little explicable through conscious activities as through blind instinct. God must endow our minds at every moment with the required perceptions, forming in us all the judgments which they involve. "At the instant that we open our eyes in the midst of a landscape, God presents to us instantaneously all the feelings, and forms in us all the judgments that we would form ourselves, if we were endowed with an infinite penetration of mind, were perfectly instructed in optics, and were acquainted with the magnitude and relation of all the images that are traced in the eyes, and in general with all the changes in the body, which can, or ordinarily do, regulate these judgments[1]." The same view is repeated more in detail in the seventeenth *Éclaircissement*. "God gives us exactly those perceptions of objects that we would give ourselves, if we had a thorough knowledge not only of what passes in our brain and in our eyes, but also of the situation and movement of our body; if, further, we were perfectly instructed in optics and geometry, and from these data......could form in an instant an infinite number of exact reasonings, and could at the same instant act in ourselves in consequence of these reasonings, giving ourselves all the different perceptions, distinct and confused, that we gain of objects by a single glance of the eye—perceptions of their magnitude, of their figure, of their distance, of their motion or rest, and of all their diverse colours[1]." "But as we were not created in order to occupy ourselves with things sensible, and to be wholly engaged in the mere preservation of life, God has spared us all this labour, and reveals to us, by an abridged and very agreeable method, in a moment, an infinite variety of truth and beauty[2]." Thus while the perceptions are 'natural,' existing in us simply as compound sensations, in reference to God, who in forming them interprets to us our bodily states, they may be regarded as genuine judgments.

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[1] *Réponse*, I. § 9.

The fact that God in exciting the appropriate perceptions in our minds always obeys the fixed laws that regulate the relation of soul and body, explains why it is that only *sensible* apprehension of distance brings about increase in apparent magnitude. "The inequality of distance must be actually perceived by the senses. Since the knowledge that we gain from other sources produces no change in our sense-organs, it does not affect our sensations. For God, in consequence of the laws of the union of soul and body, acts in our minds, making us see objects, only on occasion of the images that are traced in the eyes, and of the changes which are produced in the brain[3]." "For example, although I know with certainty that the sun is several million times larger than the moon, I yet see it as almost of the same size, since its image on the retina is equal to that of the moon, and since, further, nothing takes place in my eyes which can reveal to me the difference in their distances. On the other hand, although the image on my retina of a child 10 feet distant is equal to that of a giant 30 feet distant, I yet see the giant as three times larger than the child, because something takes place in my eyes through which I can discover the difference of their distance, such as the varying disposition of my eyes in focusing them, or the image of intervening objects......Thus knowing the distance between the child and the giant, and having also, according to our supposition, a perfect knowledge of optics, which teaches me that the images on the retina should diminish in proportion to the distance of the objects, I give myself a perception of the giant as being three times taller than the child. The reason of all this is that the eyes being formed for seeing, we must find in the changes which take place in them, and through them in the principal part of the brain, and not in anything else, the occasioning cause which determines God to act in our mind and to make us see the objects that surround us[1]."

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[1] *Dernier Éclaircissement*, § 26.

[2] *Réponse*, I. § 10.

[3] *Réponse*, I. § 4.

That, then, is Malebranche's theory of vision. Like Berkeley's theory of a divine visual language, it forms an essential part of a general metaphysical system. In Malebranche's view, it is chiefly important as an illustration of the manner in which our sensations are interpreted, as he himself says, 'in a thousand different ways,' by natural, involuntary, irresistible judgments. These judgments are frequently false from the point of view of science[2], though necessary and admirable as means to the practical organisation of life; and thus reveal at once the illusoriness of sense-experience, and its wonderful adaptation to its practical end. We cannot, however, accept Malebranche's theory simply as he states it. There are many ambiguities in his central doctrine of natural judgments, and until these are cleared up his theory does not appear in its true light. I shall try to make his theory more definite, and to remove these ambiguities, by comparing it with that of Berkeley.

Probably all present-day psychologists will agree that the chief defect in Berkeley's theory of vision is the very external way in which he connects the signs of distance with the resulting perceptions. Sometimes he speaks as if the perceptions were inferences consciously made by the individual mind from the given sensations. At other times, when he is speaking in a manner more consistent with his general philosophy, he regards the connexion as one merely of association: the signs suggest their interpretation just as a blush suggests shame or a word recalls its meaning. The objection to the first view, which probably Berkeley would not seriously maintain, is that perceptions are fundamentally distinct from inferences. Unlike inferences, they cannot be modified at will. The second explanation is, however, equally contrary to the facts. If we apply Berkeley's own favourite mode of argument, and inquire whether, on looking into the mind, we can discover any such signs as data existing prior to our perceptions, the answer must be in the negative. We do not first apprehend the signs of distance and from them gather their meaning. The perceptions arise in consciousness, as Malebranche insists, with all the apparent immediacy and sensible force of a simple feeling of pain. On Berkeley's theory, however, strictly interpreted, we can perceive nothing but the signs. All we perceive is a plane surface[1] either in the eye or immediately in front of it. The rest is merely suggested. Just as we hear only the spoken words, never their meaning, so we see only the simple given sensations, never the ideally constructed world of perception. His sensationalistic atomism, involving an absolute separation between the visual and the tactual[2], and indeed between each sensation and every other, made it impossible for him to give any but a merely mechanical, and therefore wholly unsatisfactory, account of our developed perceptions.

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[1] *Dernier Éclaircissement*, § 27. Malebranche's complete neglect of the influence of touch-experience upon our visual perceptions may here be noted.

[2] They are always perfectly correct inferences—that is one of Malebranche's reasons for insisting upon their divine origin—from the given data, but as the data are relative to the body, the conclusions drawn need not be true of external reality.

Now Malebranche, in dwelling upon the complexity, variety, and rapidity, of the processes involved in perception, and in insisting upon their consequent supra-individual or subconscious character, undoubtedly keeps closer to the actual facts, but his theory, regarded as explanation, is subject to very similar defects. His occasionalistic position only enables him to escape, not to solve, the problem as to the connexion between the given sensations and the resulting perceptions; and it would also seem to be the immediate cause of much that is unsatisfactory in the statement of his theory. It explains, for example, the great lack of definiteness in his treatment of the signs of distance. Since the signs do not serve as data for inference to the individual, but only to God, Malebranche makes no attempt to determine how far the individual requires to be conscious of them. And closer examination of his theory, especially in the light of his later and more definite statements in the *Réponse* and *Éclaircissements*, forces us to the conclusion that, on his view, it is a matter of indifference whether or not the signs are apprehended at all. The real data upon which God bases the perceptions are the brain-states, and all that is necessary, therefore, is that the signs should exist as modifications of the brain.

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[1] Though this is explicitly denied by Berkeley (*Essay on Vision*, § 158), his whole argument really rests on this assumption.

[2] A separation which had, however, very fruitful results. It brought clearly into view the problem of the interconnexion of the senses, and especially of the part which touch plays in the development of visual perception. Malebranche's failure to realise this problem is, as I have already noted, one of the chief defects in his theory of vision.

According to the laws regulating the connexion of mind and body, these brain-states must have their mental counterpart, but that counterpart may only be the interpretation, not the actual signs. Consider, for instance, Malebranche's account of the first sign of distance. Though his words seem to imply that the individual apprehends distance by a consciously thought out geometrical inference, it is apparent, when his complete theory of natural judgment has been stated, that such is by no means his intention. To Berkeley's argument that introspection does not reveal the existence in consciousness of any such geometrical reasoning, he would cordially agree. The convergence of the eyes, with the consequent brain-modifications, are the facts in the light of which the natural judgment of distance is formed in us, but that judgment arises in consciousness simultaneously with the convergence of the eyes, not as a conscious inference from it.

A second obscurity in Malebranche's theory arises from the same cause. It is difficult to see what part is left for individual experience in the development of perception. Malebranche makes it clear that in the use of apparent magnitude as a sign of distance, God does not begin to act until the individual has acquired experience of the real size of the objects; but in all other cases he leaves the part played by personal experience altogether out of account. The signs of distance are signs and occasions only to God, and are either unexperienced or else undeciphered by the finite individual. Indeed, the individual's power to interpret them, even should he possess such power, would count for nothing. Such interpretation would be purely intellectual, and sense-perception is a sensational process. A further reason for this ignoring of personal experience and of individual action lies in Malebranche's conception of the self. He interprets its nature in the light of his fundamental metaphysical principles—"Dieu fait tout en toutes choses": "Dieu n'a pas fait d'entité liante. C'est lui qui lie." The self, since we cannot know its true nature, may, he admits, possess many powers and faculties, of which, in our present ignorance, we can form no conception, but in accordance with the above principles he entirely ignores this possibility, and reduces it to an empty abstraction. Its whole essence consists in its passive capacity to receive the illumination of God[1]. It has no retentive or assimilative powers, and only appears to preserve the fruits of past experience, because God recompenses it more profusely in proportion to its past acts of attention. Taking such a view of the self, Malebranche was bound to minimise the element of personal activity, and to neglect as unimportant the processes involved in the development of experience. And consequently he falls far short of Berkeley as regards definiteness and thoroughness in the treatment of the more empirical aspects of the problem.

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[1] Cf. M. Joly's excellent discussion of this point in his *Malebranche*, pp. 208—214.

But the chief reason why Malebranche is forced to resort to a purely occasionalistic explanation of perception has still to be mentioned, namely, his Cartesian dualism of thought and sense[1]. Since thought deals only with the concepts of pure understanding, it cannot organise the independent data of sense; and perception must therefore be purely sensational, if it is to be possible at all. In the later statements of Malebranche's theory[2] this is clearly recognised, and in consequence all the diverse elements in perception are reduced to the same level as the purely sensible qualities of colour and heat. The individual does not from a knowledge of his brain-states determine what his sensations will be; and he is equally unconscious of the processes involved in the formation of perceptions. And this analogy between sensation and perception holds still further. As it requires divine knowledge, that is to say, acts of judgment and reasoning on the part of God, to determine for each complex movement of the sense-organs and brain the corresponding sensation, the same divine action, exercised in the same manner, though more complexly, determines the compound sensations, corresponding to the more complex apprehension of distance, figure, and magnitude[3]. Thus sensations and perceptions alike involve judgment and reasoning on the part of God, and may therefore with equal justice be named natural judgments. But if, on the other hand, we mean by the phrase 'natural judgment' to indicate any mental activity, any process of mental interpretation, exercised by the finite individual, neither has any right to be so described.

Now when this important point, that perceptions are as immediate as sensations, and also are nothing but sensations, is clearly recognised, the ambiguous phrase 'natural judgment' ceases to signify (though such a connotation is essential to Malebranche's theory) anything additional to sensation through which it may be objectified or acquire meaning. Let us consider Malebranche's own statements. "Since the senses can only feel, and properly speaking never judge, it is certain that this judgment is only a compound sensation[1]." "Though, conforming to ordinary speech, I have attributed these judgments and reasonings to the mind, I have at the same time named them *natural*, in order to mark that properly it is not the mind that executes them, but the Author of nature in it and for it; and I have been compelled to speak of these judgments, because our sensations, which presuppose them, and necessarily depend upon them, cannot otherwise be accounted for[2]." But, as we have just seen, these statements may be made with regard to any sensation or feeling whatsoever, and therefore do not prove, as Malebranche by his ambiguous language seeks to imply, that the individual mind in perceiving forms any judgments at all. And when that has been granted, when it is admitted that no thought processes, no acts of judgment, are involved in perception, his explanation of the apprehension of external reality—not to mention distance, figure, and magnitude—falls to the ground.

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[1] Though Malebranche explicitly declares that thought and sense are not two separate faculties, but simply different forms of understanding, two modes of perception, these statements are not borne out by his detailed account of sense-experience, nor are they in agreement with the general tendency of his philosophy.

[2] Cf. *Dernier Éclaircissement*, especially § 43.

[3] Malebranche's theory thus corresponds to the more modern attempts to regard our developed perceptions as psychological ultimates, and as explicable, like sensations, only through their physiological conditions. Malebranche would doubtless hold that though perceptions are compound sensations their simple components have fused to form a unique product.

Though Malebranche, therefore, as might be expected from his rationalistic leanings, sees more clearly than Berkeley the intellectual processes involved in perception, and admits that in reference to God perception presupposes genuine acts of reasoning, his so-called natural judgment is a hybrid form which can have no real meaning in his dualistic philosophy. He likewise names it 'confused reasoning[3],' but it is, as he candidly admits, simply a compound sensation. To have genuine meaning, it would imply that interconnexion of thought and sense which his general principles compel him to deny. Mediate processes are involved not only in the generating of perceptions, but also in their essential constitution; and a theory, such as Malebranche's, which seeks to reduce perception to sensation, leaves them wholly unexplained[4]. His occasionalistic position only enables him to conceal, not to solve, this fundamental problem.

Spite, however, of all shortcomings, Malebranche's theory of vision was a great advance upon that of his predecessors, and though by no means equal in historical importance, or in intrinsic value, to Berkeley's more comprehensive theory, it yet forms a very genuine anticipation of it. In some points, as I have tried to show, Malebranche even makes an advance upon Berkeley. He keeps closer to the actual facts in his description of our developed perceptions, and in treating of visual magnitude he in some respects shows better appreciation of its purely relative, inferential character. Malebranche therefore deserves full recognition as Berkeley's forerunner in opening out these new problems. Berkeley had certainly read the *Recherche*, but whether or not he was influenced in this matter by Malebranche's teaching, it seems impossible to decide.

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[1] Already quoted, p. 192, note.

[2] *Dernier Éclaircissement*, § 43.

[3] Cf. *Recherche*, liv. I. chap. x. § 6, note.

[4] Simple sensation, if it is to mean anything at all, must mean a simple quality. Perception, on the other hand, presupposes forms of relation.

process. The evidence of time-sequence must always be in the main indirect. The following pages attempt to collect and classify the material required to construct an adequate theory of the development of the comparison-process, and to determine the sequence in time of its different forms. This material consists entirely of the recorded expressions of comparison-judgments by speech and by gesture, employed by civilized and by primitive man, by normal persons and by deaf-mutes.

The claim to base valid inferences as to the development of psychical processes on the development of linguistic forms stands or falls with the familiar assumption that "speech is a more or less adequate mirror of thought." So general and so vague a proposition no one would be prepared to deny. It is a common-place that speech moulds thought, and that thought moulds speech. But the closeness of the relation must not and need not be exaggerated. We must reject as quite devoid of foundation the notion that to each type of language corresponds a type of thought (Aryan, Ural-altaic, &c.). Moreover, it is impossible to assert that linguistic forms at any time reflect precisely contemporaneous forms of thought. There is no doubt, however, that psychical development, while it always precedes, invariably tends to bring about linguistic development; and the only assumption required for psychological purposes is that there is a parallelism of development between thought and speech: *i.e.* that in actual linguistic forms are crystallized the results of past psychical process.

The linguistic materials which the psychologist will have to interpret in the light of this assumption fall into three groups. In the first place he would turn to the development of speech-forms in the normal individual. Such data, did they exist, would be of inestimable value. Unfortunately, the systematic study of child-language is still in its infancy, and I have been unable to discover in the extant literature of child-psychology a single fact bearing directly on the development of the expression of comparison-judgments. Next in importance is a study which has been almost equally neglected hitherto, in spite of its practical value for special paedagogy: I mean the study of natural gesture-language, whether it be that of civilized deaf-mutes, or that of primitive peoples. Psychologists and philologists too are pretty well agreed that natural gesture-language represents the earliest stage in the development of means of expression, and that in natural gesture-language the connexion between thought and utterance, between psychical process and syntactical form is much more direct than it is in speech. The gesture-language of the deaf has the additional merit of allowing us to investigate a case of arrested development, due to abnormal conditions. The results of special enquiries which, in the absence of previously-collected material, I have been obliged to make, are set forth at length in the following section.

Finally, we must call in the aid of comparative philology, as that science is usually understood. And since we desire to trace the development of forms from their earliest ascertainable stage, we must not confine ourselves to the consideration of so elaborate and highly-developed a family of languages as the Indo-Germanic. It is necessary to make as exhaustive a survey as possible of all the different types of speech, living and dead. In so novel an enterprise the mere psychologist is likely to make a good many errors of detail. Very few primitive languages, indeed, have been analysed by philologists with anything like the fulness which, from their supreme psychological importance, they undoubtedly deserve. A comparative treatise of Amerind has yet to see the light. Our information respecting most of the languages of the African continent is deplorably meagre. The Finnish-Magyar type of agglutinative languages still awaits systematic comparative treatment on an adequate scale. And so the psychologist is obliged to collect his material from scattered sources, where and how he can. The rude beginnings of such a collection are offered in Section III., the main use of which, I imagine, will be to show how much remains to be done.

II. THE EVIDENCE OF GESTURE-LANGUAGE.

A. *The Natural Gesture-Language of Deaf-Mutes.*

It need hardly be said that what the psychologists are agreed in calling the *natural* gesture-language of deaf-mutes alone calls here for detailed treatment. The study of any artificial system of signs, invented 'de toutes pièces' by the teacher and imposed upon the pupil, would be less instructive for my purposes than even the study of Volapuk. But is there a natural gesture-language? The quarrel between partisans of the manual method and partisans of the oral method of training deaf-mutes has enveloped the whole topic of deaf-mute gesture-language in a fog of confusion and equivocation. The very vehemence with which the one school asserts and the other school denies the existence of a natural gesture-language suggests that both import into an ambiguous epithet an ethical significance which for the theoretic enquirer is entirely irrelevant. From the fact that uneducated deaf-mutes employ, to express their needs, feelings, ideas, gestures more or less akin to, yet infinitely more elaborate, subtle and various, than those used by normal persons, the one school of pedagogues argues that such is the kind of language which is 'natural' to deaf-mutes, and that, being 'natural,' it must needs be the best and should be used as the chief means of educating them. The other school denies the validity of the inference, which in itself is certainly weak enough. But in the heat of controversy, they confuse the issue and, rather disingenuously, distort plain facts concerning which no serious doubt can be entertained. It is unquestionable that deaf-mutes have a large number of gestures at their disposal, which they have never learnt from any teacher, which they habitually use for intercourse with other deaf-mutes, and which, even after years of oral training, they have recourse to when under the influence of excitement, or when the restraints of school-regulations are temporarily in abeyance (in the playground, and after school-hours). These gestures are natural in the sense, (i) that they do not need to be acquired by formal teaching, (ii) that they are immediately intelligible, (iii) that they are in the main common not only to deaf-mutes of all countries, but also to primitive peoples among whom sign-language is strongly developed, (iv) that they constitute the first method of expression to make its appearance. It is with such gestures alone that we shall be concerned.

The analysis to be presented in the following pages is based upon the answers to a questionnaire widely circulated in 1902–3 in France, Germany, Italy, England, and the United States of America. To fix the ideas I print here a list of the points (relevant to this paper) on which information was solicited.

1. How do deaf-mutes express by gestures identity or difference in general?

2. How do deaf-mutes express by means of gestures judgments such as the following?

(α) Judgments involving space-relations: size, distance, &c.

(I) Comparative: *e.g.*

A is larger (or smaller) than *B*. (i) when the difference is quite striking.

A is farther away (or nearer) than *B*. (ii) when the difference is just perceptible.

(II) Superlative: *e.g.*

Of several objects *C* is the largest (or smallest).

Of several objects *C* is furthest away (or nearest).

(β) Judgments involving estimation of muscular effort.

(I) Comparative:

A is heavier or lighter than *B*.

(i) when the difference is quite striking,

(ii) when the difference is just perceptible.

(II) Superlative:

Of several objects C is the heaviest (or lightest).

(γ) Other judgments (moral, aesthetic, &c.) such as:

My frock is prettier (cleaner, older, &c.) than hers.

My frock is less pretty (less clean, less old, &c.) than hers.

This picture is more beautiful (or less beautiful) than that.

Tom is a better boy than Harry.

These jejune instances, and more of the same familiar type, were meant to indicate the different kinds of judgment of which I required descriptions. My informants were also requested to state:

3. In what cases would the deaf-mute use for the expression of comparison-judgments:

(i) the simple assertion of the quality in the case of A, and simple denial in the case of B,

(ii) the sign of the quality of the object compared, together with the sign for 'more' or 'very' (or 'less,' or 'little'),

(iii) or any other means?

My informants were encouraged to enumerate and describe all the methods with which they were acquainted of expressing comparison-judgments by natural gesture-language, and to state which method was, in their opinion, the earlier in the development of the child's means of expression.

The analysis now to be presented follows the order of the questionnaire[1].

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[1] My obligations to each informant will be acknowledged specifically in the proper places. But I wish here to place on record my gratitude to all who were kind enough to assist by their painstaking contributions in a difficult enquiry. Without the generous help of Dr Alexander Graham Bell and of the 'Volta Bureau, Washington, for the increase and diffusion of knowledge relating to the deaf,' through which some of my most valuable answers reached me, the results of my enquiry would have been very much more incomplete than they are. Special thanks are also due to Dr N. Ach, of Göttingen, who kindly carried out at my request a lengthy investigation at the Würzburg Taubstummenanstalt; to Prof. G. E. Müller, for a valuable communication from Herr Ulbrich of Breslau; to Prof. F. Schumann for a communication from Herr Mahner of Berlin; to Prof. H. Bergson for an authoritative paper by M. Boyer, Professor at the Institution Nationale des Sourds-muets de Paris.

The Expression of Identity or Difference in general.

(*a*) The most primitive method is assuredly the following:

"The deaf-mute endeavours to place the two interesting objects side by side, and leads his interlocutor up to them; touching the objects one after the other, he makes a sign of emphatic denial if *A* differs from *B*, of emphatic assertion if *A* and *B* are the same." (*Giulio Ferrero.*)

"The deaf-mute points with his right forefinger towards one object, with his left forefinger towards the other, gazes alternately from the one to the other with slight movements of the head as if engaged in looking from *A* to *B* very intently. If the two objects are different, the sign is concluded by a vigorous shake of the head indicative of a negative judgment, *No*, or *Not*, and interpreted as not alike or not equal. Identity, or similarity is expressed by a vigorous nod indicative of an affirmative judgment = *Yes, same, alike.*" (*J. C. Gordon.*)

It matters not whether *A* and *B* have only one attribute in common, or all attributes with the exception of space-relations. For two apples whose size is the centre of interest, for two pieces of chalk alike or differing merely in colour, for an elephant compared with a turtle, the essential principle of gesture-expression remains the same: difference or identity is asserted in the lump; the two objects are radically the same or radically different. Some one attribute which *A* possesses and *B* does not, or which both *A* and *B* agree in possessing, obscures the consciousness of underlying identities or differences.

At a later stage, assertion and denial are still made concerning the objects as wholes, and without any expressed analysis of them into their attributes, but there are infinite gradations in the intensity of the facial and pantomimic expression of assertion and denial. Secondary characteristics, not explicitly stated, perhaps not clearly apprehended, modify the confidence with which the judgment is delivered. This is a transitional stage which is none the less important for being so subtle and difficult to illustrate by means of any one concrete instance.

(*b*) The next stage is that in which the judgment is no longer of the object as a whole, but of some particular attribute of the object. "The deaf-mute insists on first indicating the quality of the two objects, in respect of which he is to compare them, so as to draw the observer's attention to it. The rest is done by natural mimic as described above." (*Giulio Ferrero.*) After the sign for the relevant quality, the deaf-mute points to each object in turn, denying or affirming the quality, as the case may be, of either object. The magnitude of the difference is expressed by the intensity of the facial expression, or by the energy and extent of the pantomime.

Thus "a more conventional form of gesture for expressing difference is as follows: place both forefingers side by side, the other fingers being closed, and the backs of the hands turned upwards, then move the hands away from each other, the forefingers pointing in opposite directions. The degree of difference is expressed by the rapidity and extent of this movement. A rapid and full sweep of the two fingers until the arms are extended at full length, if accompanied by an earnest gaze, means 'wide as the poles asunder.' A slow, hesitating movement, with a look of uncertainty, or of contempt, indicates a small difference, only just noticeable, of no special interest." (*J. C. Gordon.*)

Here again we are to note the delicate nuances of meaning which pantomime alone is able to express. In discussing the psychological nature of sameness and difference, evidence such as this is very important. Psychologically speaking, one may assert, and it is obvious that the deaf-mute does assert, a proposition with any number of degrees of intensity. Even if he does explicitly announce the attribute in respect of which he is comparing two objects, the deaf-mute in his gesticulations does not assert with constant vehemence its absence in the one case, its presence in another. Whereas logically speaking, assertion and denial are rigid contradictories, psychologically speaking they are not. Each admits of an infinite number of gradations. Now the varying intensity of assertion plainly implies much more than varying certainty of judgment. One may indeed be quite as confident in the case of a small as in the case of a large difference; equally confident too in the one case *that* it is a small, in the other case *that* it is a large difference. A comparison-judgment of two objects in respect of one single, simple, determinate, and determinately apprehended attribute would conform to the logical ideal, and would not admit of varying intensities of assertion. But in common experience such attributes are rare, and the lower the level of psychical development, the more scarce they are. The apprehension of a difference or an identity between two complexes does not imply that each of the constituent elements of the one is discriminated from all the constituent elements of the other. The deaf-mute, as indeed every other person, frequently bases a judgment upon a quality which is apprehended as a whole, whose elements are not clearly distinguished. In the earliest phase (method *a*), while his attention is riveted upon one aspect of an object of absorbing interest, he does not explicitly distinguish between the whole and the determinant part:—the part *is* the whole for him (this is shown by the fact that his signs for a given object vary according as he is interested in one quality or in another). In the next phase (method *b*), although he does single out an attribute, it is by no means completely analysed. The predominant aspect alone is focused, the rest remains obscurely undifferentiated. This want of differentiation, together with the recognition of latent determinations, finds expression in the varied intensity of assertion and denial.

The Expression of Judgments involving Comparisons of Size or Extent.

Method (*a*). An attribute which is asserted of one object is denied of another. So far as judgments of size or extent are concerned, this method appears to be used only when the difference between A and B is for some reason striking. To express $A > B$ if the difference is considerable: "the deaf-mute first makes the sign for *large*, by raising his hand, and looking upwards with an expression of admiration, then the sign for object A, with a nod of affirmation. He ends with the sign for B and shakes his head in denial, with an expression of contempt." (*Boyer.*) He uses, therefore, two propositions, first asserting that A is large, and then asserting that nothing of the sort can be predicated of B. However, M. Boyer remarks, the intention of comparing is clearly before the mind of the child. He does not mean that B is absolutely small and contemptible in itself; indeed, if placed beside a third object C smaller than B, he might now assert that B is large, while C is not. The quality possessed in different degrees by A and B, or B and C, is denied of the one only relatively to the other.

Similarly to express $A < B$, although the deaf-mute would generally prefer in M. Boyer's experience to reverse the judgment, he will sometimes make the sign for small (lowering the hand towards the floor with an expression of contempt), then the sign for A, and then he will deny smallness of B.

Method (*b*). "The palms of the hands are turned inwards, facing one another. The distance between them indicates size. The greater this distance, the greater the size indicated. Similarly, the distance between the palm of the hand turned downwards, and some point of reference, the table, or the floor, indicates height. To differences in actual height correspond differences in the indicated distance. At the same time the gesture is supplemented by the facial expression, gradations of which run parallel to gradations in the extent of movement, from the expression of astonishment or admiration when the size indicated is large, to that of contempt when it is small. Each proposition embodying a judgment of comparison of the type $A \gtrless B$ requires then two separate statements in the language of gestures: A so large. B so large. The *terminus a quo* is always stated first." (*Ulbrich.*)

With this account of the matter all my correspondents are in fundamental, often in literal agreement. Thus: "Degrees of comparison are easily expressed, *e.g.* a circular movement of the right hand and forefinger may mean *small* basin, a rather larger movement of the same kind will mean *larger* basin; and to express the largest basin, say of many, a similar large movement with the additional sign of placing the left-hand palm on the tip of the right-hand forefinger, held upright, denoting a crown or top, above, or greater than all, &c." (*W. Nelson.*)

Or again: "The hands are placed at an appropriate distance apart, or space is indicated upon the left hand and arm by the right hand, accompanied by facial expression of varying intensity. The space circumscribed, and the facial expression change as the size of the objects signed changes. The magnitude of the difference between the objects is expressed by the difference in the extent of the movements by which the size of each object in turn is indicated." (*W. H. de Motte.*)

"Expressed in words, the gestures for $A \gtrless B$ mean: A so large, B so large. It is rather a coordination of two separate statements than a comparison, although the intention of comparing is before the child's mind." (*Boyer.*)

We may summarise the above evidence in a general rule. When two objects are compared in respect of their size, one of the methods by which the deaf-mute is able to express a judgment is as follows: the size of each of the objects in turn is indicated by means of a true imitative gesture, and it is the absolute magnitude, not the relative magnitude, which is rendered. Thus the contour of one basin will be reproduced in the air in its actual size, and then the contour of another, also in its actual size. Or again, it is the actual volume of each apple which is rendered by the hollowing of the palms.

However, if the sizes or heights to be expressed exceed the possible stretch of arms and hands this method is no longer applicable. The objects compared have to be rendered then not in their absolute, but in their relative sizes. That is to say, if A is twice as large as B, the extent of movement by which A is expressed will be twice as great as that by which B was expressed. The movement expressing B is no longer strictly imitative in the most primitive sense, for its extent is to some degree arbitrary. A certain extent of movement is chosen to represent the size of B, although it does not reproduce the size of B. Still, each object is expressed in and for itself, and we are still bound to translate the gestures as before. A so large, B so large. This important step in the development of purely imitative into representative gestures is also a step in the development of the synthetic judgment of comparison out of the coordination of two independent judgments. This change reaches completion only in certain types of spoken language. But we can discern signs of it in the gesture-language of deaf-mutes.

Method (*c*). For indeed "Besides the above-mentioned gestures expressing absolute extents one after another, there is another class, consisting in a gradual movement of the hands towards or away from each other from an initial position, which is more or less arbitrary. A certain distance apart of the hands is chosen to represent the size of one of the objects, and this distance is increased or diminished gradually. Translated into words, such judgments mean: *A* so large, *B becoming* so large. The size of *B* is not rendered in and for itself, but in terms of *A*, and the whole point of the gesture is that it is meant to express the growth or increase observable in *B* when one starts from *A*." (*Ulbrich.*)

My informant adds further that whereas gestures of type (*b*) are capable of indicating only the absolute size of each of the objects in turn, the gestures of type (*c*) emphasize the relation between the magnitudes of the objects. It is plain that these signs for larger and smaller appear to indicate a *becoming* larger or smaller. How are we to interpret the notion of change or transition which they seem to imply? Herr Ulbrich thinks there can be no doubt that the deaf-mute thinks of a *being*, rather than of a *becoming*, larger or smaller. To the plain man it seems evident that two objects, once given, do not change, according as you look first at one or at the other of them. We shall have, therefore, to guard carefully against offending common-sense, when we come to interpret the notion of transition which underlies so large a part of man's attempt to express the fact that one object possesses more (or less) of a certain attribute than another. All we can do here is to draw attention to the admitted facts, without insisting upon the interpretations that might be given to them.

The highest point in the development of natural gesture-language is reached when for the co-ordination of two separate statements is substituted the partial synthesis just defined: when the sequence of relatively independent and discrete imitative gestures expressing by the extent of movement the absolute magnitudes of the objects under consideration has made room for the sequence of relatively interdependent and continuous representative gestures expressing relative magnitudes by means of increase or decrease in extent of movement starting from an arbitrary initial position.

The view here taken of method (*c*), that it is the last stage in a long and gradual development, hardly needs special justification. We know that there are four main stages in the growth of gesture-language: the indicative, the imitative, the representative and the symbolic. We can trace the development of each of these stages out of the stage immediately preceding it, and it is enough to know in the case of any particular gesture which of the four classes it belongs to in order to form a probable opinion as to the date of its origin. The gestures we have discussed are not pure types, and perhaps their main interest lies in the fact that they constitute transitional links between the various classes. We can see at any rate that the gestures described under (*b*) belong rather to the class of imitative signs, whereas those described under (*c*) belong rather to the class of representative signs. This is a sufficiently sure criterion. Our view is confirmed by the fact that method (*c*) is very rarely employed as compared with method (*b*).

Method (*d*). It is important to note in this connexion that the notion of *increase* (or of *decrease*) underlies the most important of the gestures by means of which the deaf-mute expresses such ideas as 'more,' 'less.' To signify these, symbolic gestures are used. Thus the deaf-mute signs *A* large, *B* larger (the sign for the attribute being the same in both cases). But if he wishes to express the superiority of *B* in point of size, he will make an additional sign signifying more. It is as follows: "the right hand with fingers outstretched is gradually raised, and the face expresses increase, by gradual intensification of various expressions, admiration, wonder, satisfaction, &c...." The sign for less is just the reverse: "instead of raising the hand, lower it towards the ground, with a facial expression of decrease." (*Mahner.*)

The difference between this method and method (*c*) appears to be that whereas in (*c*) it was the attribute itself which was represented as increasing, and all the movements for *larger* (or *smaller*) were parts of one single gesture belonging to the representative class; in (*d*) the attribute itself is not represented as increasing, and the movements expressing larger or smaller constitute two separate gestures, both symbolic, the one meaning *large* (or *small*), the other meaning *more* (or *less*). Whereas then we had to translate the judgment described under (*c*) in the terms *A* so large, *B* becoming larger; we must translate the judgment described under (*d*) in the terms *A* large, *B* large—but more.

It is easy to see how the one method developed out of the other. And it is obvious that (*d*) is a later step in the development of expression than (*c*). According to a very competent informant, "signs for *more*, or *less* do not occur in the gestures of young children until after prolonged intercourse with older deaf-mutes." (*Ulbrich.*) At first, we may surmise, the unitary representative gesture and the pair of symbolic gestures were in their use and import well-nigh indistinguishable. It would be possible to indicate by means of either of them equally well the relative extent to which a certain attribute belonged to two different objects. In both cases the relative extent of hand movement whether vertically or horizontally would suffice. But there are many attributes, not involving space-relations, the gestures for which do not admit of any variation in extent of movement. To these the gesture expressing increase of volume or length was extended. This new use hastened the disintegration of the old unitary representative sign until the complete separation of the sign for the attribute taken in an absolute sense, and the sign for *more*, taken just as absolutely. At this stage it is no longer possible by method (*d*) to express the relative degrees in which two objects possess a common attribute. Gesture-language acquires something of the rigidity of speech. What it gains in abstraction, it loses in completeness and compactness. The sign for *more* (or *less*) involving the notion of spatial increase or decrease, and developed from an earlier and more complete gesture applicable only to spatial relations, tends to lose more and more of its representational character, acquires more and more the character of a pure and abstract symbol, is applicable in the end to any kind of attribute. As a matter of fact, however, "it is generally restricted to adjectival signs which are not susceptible of undergoing any variation in respect of extent of movement, as *e.g.* the sign for 'old,' or 'beautiful,' or 'clean.'" And again this method "is generally used when the deaf-mute who has been educated by the oral method is translating a spoken sentence into gesture-language." (*Ulbrich.*)

The Expression of Judgments involving Comparisons of Distance.

The essential principles are the same as for the comparison of sizes, and the only justification for treating these two topics separately lies in the fact that the gestures employed in the latter case exhibit, so far as my evidence goes, more clearly and completely the filiation of successive stages in the development of gesture-syntax than do the gestures employed to express comparison of distances. A given distance is expressed by means of a certain extent of pantomimic movement, and the difference between distances is rendered by the difference in the extent of pantomimic movements by which they are successively expressed.

"Deaf-mutes at our institution indicate distance by the actual distance of the right-hand from the body, accompanied by appropriate physiognomic expression." (*Ulbrich.*)

"To sign '*far-off*,' the deaf-mute extends his hand straight away from his body, with a sort of scared look, not at all easy to describe. To sign '*close to*,' the hands, first of all, at some distance apart, are moved one towards the other." (*Boyer.*)

"At some distance: movement of hand away from the body.—Close by: movement of the hand towards the body.—Further away: same sign as for far, only stronger. Comparative and superlative are conveyed by varying extents of movement." (*Ach.*)

"*A* is at some distance: the child points easily towards the objects. *B* is farther off: the child points as before with more or less strain of the muscles of the arm and contraction of the muscles round the eyes, as if he were gazing far ahead. Farthest off: same motion and expression intensified." (*W. H. de Motte.*)

"*B* is far: the hand points a long way off from the body. *A* is further: the motion is continued, the extent of movement being larger in the one case than in the other."

The Expression of Judgments involving the Estimation of Muscular Effort.

The deaf-mute expresses weight by means of pantomime and physiognomic expression. He makes the gesture of lifting an object with one or both hands from a support, or of letting it down gradually from a certain height. Variations in the weight expressed are rendered by variations in the rate of lifting and in the physiognomic expression of effort. Thus in answer to the question 'which jug is the heavier?' the deaf-mutes examined by Dr Ach gave the following gestures: "Hermann lets the heavier jug held in one hand sink lower and lower with every expression of effort in the twitching of his facial muscles, in the bending of his knees. The lighter jug he holds up steadily, with an expression of facility, ease, comfort on his face. Krug points to the lighter jug and lifts his hands quickly upwards. Eisner's sign for light is to point to the lighter jug, and then, palms upwards, to raise his hands several times in succession. For heavy, he points to the heavier jug, and lets his hands drop." Thus just as in spoken language light and heavy stand for opposite attributes rather than for different degrees of the same attribute, so in gesture-language, there is an even more obvious opposition between the sign for heavy and the sign for light. The comparative is frequently expressed by predicating opposite attributes of two objects: *A* heavy, *B* light. This appears to be in gesture-language, just as in ordinary speech, the most primitive method. The next stage is that in which the progress of abstraction and analysis has brought into clear consciousness the identity underlying differences, the continuous gradations of qualities. The gestures may now be translated in the terms *A* so heavy, *B* so heavy, the extent and intensity of pantomime being the same in both cases if the two weights are equal, proportionately different if the weights are unequal. There is a transition to the symbolic when to the more intense pantomime expressing the heavier weight there is added some further mark of emphasis, as for instance clapping the back of the right-hand against the palm of the left.

The Expression of other Judgments.

There remains a large class of qualities the gestures for which do not involve any reference to space or time-relations. Differences in magnitude are expressed by differences in the extent of pantomimic movements; differences in weight by differences in the rate of pantomimic movement. In both cases to the differences in extent of rate of movement there correspond differences in the intensity of physiognomic expression. In many other cases, while physiognomic expression may still be susceptible of increase or decrease in intensity, there is no room for variation in the extent or the rate of pantomime. In yet other cases there is little or no room for physiognomic variation, and some other method must be used to express the relative increase or decrease of a quality in one object as compared with another.

Take for instance the gestures for beautiful. The most primitive is probably the following: "the child looks at the beautiful object with an affectionate smile, and strokes it with a lingering caress." (*Boyer.*) This is probably the origin of the well-known conventional gesture in which the thumb and forefinger extended are moved downwards over the face. The face as symbol for the person becomes the symbol for every interesting object. The caressing movement carried out on all accessible objects becomes the symbol for that which is pleasant to the touch, agreeable to the eye, beautiful. The natural expression of spontaneous affection is transformed into an abstract symbol applicable to the idea of beauty in its most generalized form. Once the sign has assumed the symbolic character, the only way to express the more or less of the quality symbolized is by means of the greater or lesser intensity of physiognomic expression. To render '*A* is more beautiful than *B*' in symbolic gestures, the deaf-mute will either (*a*) assert that *A* is beautiful but *B* is not: he will make the gesture for beautiful, and smile with satisfaction and desire when *A* is mentioned. He will make a grimace of contempt, disgust, or dislike, and turn away his head when *B* is mentioned; or else (*b*) he will make the same symbolic gesture in the case both of *A* and of *B*, but he will express physiognomically more satisfaction, his mouth will expand into a broader smile, his eyes brighten with a look of greater affection when *A* is mentioned than when *B* is; or again (*c*) he will make the same symbolic gesture in the case both of *A* and of *B*, but he will supplement the physiognomic expression of preference for *A* by means of the gesture signifying *more* (with fingers outspread the hand is moved upwards with an expression of admiring astonishment).

The Expression of Judgments involving the Superlative.

There are as many stages here as in the case of the comparative. We need not therefore discuss the superlative at any length. There is no difference in principle between the expression for the one and the expression for the other. It will be sufficient to tabulate the main facts:

(*a*) Affirmation of an attribute in the case of one object only; denial of this attribute in the case of all others. To express: *A is the largest of a number of objects,* "the deaf-mute indicates by raising his left-hand the quality of being big, whilst with his other hand he makes the signs for the various objects *B, C, D,* &c., denying size of all but *A*. This is an obvious extension of the most primitive expression for the comparative." (*Boyer.*)

(*b*) and (*c*). The only difference between the expression for the superlative and that for the comparative in the imitative and representational stage "consists in the stronger emphasis placed upon the relevant gestures." (*Ulbrich.*) Thus, comparing *A, B, C, D,* of which *D* is the largest, the deaf-mute may express the size of each object with imitative gestures, beginning with the smallest, traversing all the stages of increasing size, and ending with the largest. Upon this gesture he lays special stress, and supplements it with an expressive play of physiognomy. If he uses representational gestures, such as those already described under (*c*), he will start from an arbitrary standard, indicate by gradual extensions of the original gesture the successive increases in relative size, as he passes from one object to the next larger, until he reaches the largest. Here again he pauses with marked pantomimic and physiognomic stress.

The translation of these gestures will read as follows: in the first case *A* so large, *B* so large, *C* so large, *D* so large—stress upon *D* indicating the end of the series. In the second case: *A* so large, *B* larger becoming, *C* larger becoming, *D* larger becoming. There are pauses to indicate the relative sizes of *B, C* and *D* in terms of the standard. The most emphatic pause is upon *D*, once more to mark the end of the series.

(*d*) At the symbolic stage, again, the parallelism between the expression of the comparative and that of the superlative is marked and interesting. "*A*, *B*, *C* and *D* may all have largeness attributed to them, and to express that *D* is largest of all, the sign for large is supplemented by the sign for '*more*' (increase) repeated several times." (*Mahner.*)

Alternative gestures are purely symbolic. Thus *e.g.* the sign for the absolute superlative '*very*,' or the sign for '*greater than all*' (place the left-hand palm on the tip of the right-hand forefinger held upright), denoting a crown or top, above, or greater than all. (*Nelson.*)

B. *The Gesture-language of Primitive Peoples.*

From printed sources very little information can be obtained as to the gestures used by primitive peoples in general to express degrees of comparison. In fact, the only definite information I have been able to discover concerns the American Indians. Garrick Mallery, in his authoritative Smithsonian Report[1] devotes a short paragraph to the topic. The main points are as follows:

(*a*) Degrees of comparison are frequently expressed by adding the sign for 'big' or 'little' to the generic sign. Thus *damp* = wet—little. *Cool* = cold—little. *Hot* = warm—much.

(*b*) The extent, or the energy, of motion often indicates corresponding diminution or augmentation.

(*c*) Mallery mentions also that the principle of opposition as between right- and left-hand, between thumb and forefinger and little finger, which is used to express 'above,' 'below,' 'forward,' 'backward,' and such like contrasts, is also employed to express degrees of comparison. Unfortunately, he gives no details; and, plausible as the interpretation seems, it would perhaps be rash to infer from this casual remark alone that the forms alluded to (which he says are common to Amerind and to deaf-mute sign language) are identical with those we have analysed at length above: the opposition namely of big and little, long and short, &c.

Under the circumstances, I am particularly fortunate in being able to adduce some extremely valuable evidence, here printed for the first time, with which Major H. L. Scott, of the U.S. Army,—a very high authority—has been so kind as to provide me.

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[1] "Sign-language among the North-American Indians compared with that among other peoples and deaf-mutes." *Am. Bureau of Ethnology*, Smithsonian Institution: 1st Report 1881, p. 363.

In the first place, the Amerind employs a method in the expression of comparison, which we have already found reason to look upon as the most primitive of the methods used in the gesture-language of deaf-mutes, and which we shall prove to be the most primitive method in spoken languages. He may assert a quality of *A*, and deny it of *B*. Thus $A > B$. Make the sign for big and point to *A*. Then point to *B*, and make the sign of denial. This method persists in a more highly developed stage. He possesses one gesture for big, and another for small, and he may, instead of first asserting and then denying, make two positive statements: *A* big, *B* small[1].

In the second place, the Amerind frequently uses the following method: "To describe the fact that *A* is larger than *B*, point at *B* and make the sign for big, separating the hands 1 foot. Then point at *A*, and make the sign for big again, with a greater distance separating the two hands."

A third method employed currently by the Amerind does not occur, so far as I am aware, in deaf-mute gesture-language. "Another way," writes Major Scott, "would be to make use of the signs 'before' and 'after,' meaning by 'before' sign used with sign 'big,' that the bigness is in front or ahead; the 'after' sign meaning that the bigness comes behind or after, *i.e.* is less." The sign 'after = behind' is sometimes used to indicate that the object indicated falls short of what it ought to be, is inadequate.

Finally, while degrees of quantity and intensity are very often expressed by corresponding variations in the emphasis and extent of the gesture, comparison is most frequently expressed by means of additional signs. In particular, the sign 'strong' plays a very important part. Thus if any object is pre-eminent in respect of some quality, the quality is asserted of it, and emphasized by the sign for 'strong.' There is no distinction between comparative and superlative, the same sign doing justice for both. We are reminded here of the highest point to which the development of deaf-mute sign-language attains, and we shall find a closely analogous mode of expression in primitive spoken languages. The chief difference between Amerind and deaf-mute gesture-language lies in the fact that the former has reached a far higher level of relative abstractness than the latter. The 'adverbial' expression of comparison is rare in the latter, and usual in the former. Major Scott is of opinion that the Amerind gesture-language has advanced very far in the development of the abstract notion of degree[1].

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[1] The sign for 'big' is as follows: "The hands are held out in front of the body, fingers extended and pointing upward at a slight angle above the horizontal, thumbs closed and elbows near the sides,—hands about 4 inches apart. Then separate the hands in the same horizontal plane, the amount of separation depending on the idea of size it is wished to convey." To signify 'small,' "The hands are placed in the first position of the sign for large; then they are crossed, the right-hand passing above the left; the wrists are bent inward and cross each other, about 2 inches apart, the degree of smallness being indicated by the sweep of the motion and by the distance that the wrists are crossed." (*H. L. Scott.*)

C. *Summary of the Evidence of Gesture-language.*

It will be convenient, before we go any further, shortly to recall the stages through which, in its progressive development, the gesture-language syntax of comparison has been shown to pass. There are four stages which can be sufficiently clearly distinguished to justify a fourfold classification.

I. The most primitive method of expressing comparison is simply to assert the quality (in respect of which comparison is made) of one of the objects and deny it of the other. (Method of *Opposition.*)

II. In the second stage, the quality is asserted in the case of both objects, and the difference is indicated by means of the difference in amplitude or emphasis of the gesture. This gesture may be a pure imitative gesture: for instance the size of each of the two objects being accurately reproduced; or it may be a rudimentary representative gesture, in which case, alone the relative, not the absolute sizes are reproduced.

III. The third stage may be characterized in different ways from different points of view. We may speak of it as one further step towards the substitution of one single synthetic judgment, for two separate, independent and equipollent judgments; or as the development of the explicit notion of a standard of comparison. In whichever way we look at it (and it is useful to look at it in both ways) we discover that the leading conception underlying it is what we shall later call, following Ziemer, that of *Separation.* Thus in comparing objects with respect to their size, a certain distance apart of the hands is arbitrarily chosen to represent the standard. Whereupon, *starting from* this standard, the distance apart of the hands is either increased or diminished, to signify the magnitude of the comparatum.

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[1] He writes: "I have observed no difference with regard to the forms enumerated among the different tribes of the Plains east of the Rocky Mountains, and I have conversed with representatives of every tribe from British America to Mexico."

I have already described the gesture at length. I will anticipate the sequel, by warning the reader that in spoken languages we shall find innumerable parallels to this usage.

IV. Finally the third stage tends irresistibly to flow into a fourth, in which the gesture for increase as such is separated off from the gesture for quality, and becomes the nearest approximation to an adverbial expression to be found in gesture-language. The principles of Separation and Gradation which can be clearly distinguished in the more definite syntactical forms of spoken languages, are, in gesture-language, more difficult to hold apart. But while they co-exist in stage III, Separation is there much more prominent than Gradation, and in stage IV there are to be met many instances of pure Gradation.

Parallel to this development in the Syntactical forms of gesture-language, there runs a development in the elementary gestures used which has already been commented upon. In the first stage, we meet with the most primitive of all gestures, the *indicative*. In the second stage, the *imitative* gesture, and then the *representative* gesture comes to the fore. In the third stage, there is an advance towards the *symbolic*, which is completed in the fourth. Apart from specific evidence, these distinctions alone would furnish a clue as to the actual time-sequence of the development of the syntactical form. To anticipate once more, just as in highly developed spoken languages we shall discover vestiges of more primitive syntactical forms among (or as substitutes for) the more elaborate; so in the highly developed gesture-language of the Amerind we find together with an almost universal observance of the rules of method IV, vestiges of the first and the second stages, which are in every way identical with the forms of expression used predominantly by young uneducated deaf-mutes.

III. *The Evidence of Spoken Languages.*

In the present Section the different methods of expressing judgments of comparison employed by spoken languages are collected and classified. The examination of a great variety of languages belonging to all the different structural types[1] shows that all such methods can be brought under one or other of Six main principles, which I propose to call the principles respectively of Opposition, Exclusion, Separation, Gradation, Apposition, and Composition.

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[1] The classification of languages here adopted is that of Misteli. See his *Charakteristik der hauptsächlichen Typen des Sprachbaues*, Berlin, 1893, S. 109. The term Catenary which I use to translate Misteli's *Anreihende Sprachen* (the fourth class in his list) was suggested to me by Mr Giles, of Emmanuel College, Cambridge.

1. *The Methods of Opposition and Exclusion.*

Two forms of *Opposition* are to be distinguished, according as (*a*) a quality is asserted of one object and denied of the other with which it is compared; or (*b*) a quality is asserted of one of the pair of objects to be compared, and the opposite quality is asserted of the other object. These two cases have been shown to occur in gesture-language, and form (*a*) was found to be more primitive than form (*b*). The like holds good of spoken languages. Form (*a*) is only to be found in the most primitive of all, whereas vestiges of form (*b*) subsist even in the most highly developed.

Form (*a*) is found as a well-established rule in several languages of the Incorporative type. It prevails in the Amerind tongues of Mosquitos and Abipones, in Mexican and Totanaca[1].

Thus Mosquito languages[2]:

Jan	almuk,	Samuel	almuk	apia
John	old,	Samuel	old	not.

It is also found in very primitive tongues of the Stem-isolating type. Thus the Kamilaroi of New South Wales[3] say:

Num murruba— nungillia guggildul
This is good—that is no good at all.

Closely connected with this form of expression is another which occurs sometimes in languages of a higher type. The denial is however no longer explicit, but implicit. Under the rule of what I propose to call the method of *Exclusion*, it is asserted that one of the objects possesses a certain quality, and that each quality belongs to this object alone. Thus among Stem-isolating languages, various Melanesian[4] tongues (Fiji, New Britain, Mota, Motu), instead of saying, *e.g.* 'this is good, that is not good,' sometimes use the form 'this only is good.'

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[1] Pott, *Etymologische Forschungen*, Bd. I. S. 850.

[2] *Proc. Amer. Ethnol. Soc.*, Vol. II. p. 241.

[3] R. H. Mathews, "Languages of the Kamilaroi and other aboriginal tribes of New South Wales." *Journal of the Anthropol. Institute*, 1903, Vol. XXXIII. p. 262.

[4] See Codrington, *Melanesian Languages*, Oxford, 1885, *passim*.

So too, in the Australian tongues of *Eaglehawk and Crow*[1]. A similar usage may be instanced from a tongue of the Catenary type, *Cinjandza*[2]:

Madzi ni čakudia, komo čakudia
Water and food, food good.

The objects to be compared are enumerated, and the quality in respect of which they are compared is asserted of one of them to the exclusion of the other.

Form (*b*) of the method of opposition occurs with much greater frequency than form (*a*). Among Incorporative languages, that of the Baurs of Bolivia[3] yields an instructive instance. As a rule they use expressions of the type:

this man knows little, that man knows much;
or, the father is clever, the son is stupid.

Among Catenary languages, that of the Damaras offers a pure type. Speaking of them, Galton says: "They have no comparative in their language, so that you cannot say to them: 'Which is the longer of the two, the next stage or the last one?' You must say: 'The last is little, the next, is it great?' The reply is not: 'It is a little longer,' or 'very much longer,' but simply: 'It is so,' or 'It is not so[4].'"

In the Stem-isolating languages it is one of the two principal ways of expressing comparison. Several instances deserve to be given[5]:

Micronesia. *Ponape, Caroline Is.* memeu menen, mejuit meten
good this, bad that.

Polynesia. *Samoan.* e lelei lenei, a e leaga lenā
is good this, but is bad that.

" *Futuna; Horne Is.* e ikiiki au, kae lasi a Petelo
am short I, but tall Peter.

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[1] John Mathew, *Eaglehawk and Crow*, London and Melbourne, 1899: "One mode of comparison is to single out an individual and say of it 'this (is the) large,' or 'this (is the) good.'"

[2] Ziemer, *Vergleichende Syntax der indogermanischen Comparation, insbesondere der Comparationscasus der indo-germanischen Sprachen und sein Ersatz*, Berlin, 1884, S. 185.

[3] Ziemer, *loc. cit.* S. 10.

[4] Galton, *Tropical South Africa*, p. 132.

[5] Many of the following instances are copied from a communication "On the Expression of Degrees of Comparison in the Languages of Oceania," with which Mr S. H. Ray, a well-known authority on the subject, has been kind enough to provide me.

Melanesia[1]. *Santa Cruz.* qoi ka lepu, like ka topa
pig is big, rat is small.

" *Nifilde.* poe e lo, lapu laki
pig is big, rat small.

" *Mota, Banks Is.* iloke we wia, ilone we tatas
this is good, that is bad.

" *Epi, New Hebrides.* nai te teliki, nai na tone
thing this little, thing that big.

Similarly in Pentecost, Florida Gela, and in Duke of York Island, it is more idiomatic to express comparison by the opposition of contrary epithets than by any other means.

According to Mr Ray this usage is also found in some of the languages of Indonesia, but is not very common.

The same usage is found in various Australian tongues:

Thus *Kamilaroi*[2]: murruba numma, nungunage guggil
this is good, that is bad.

Pitta-Pitta[3]: kooyungo-yetcha nunja, munna-yetcha impa
good-more I am, bad-more you are.

In Papuan, as a general rule, comparison is expressed almost exclusively by means of the opposition of two contrary epithets, or of two contrary sentences. Thus in the Miriam language[4]:

Debele ma ededem barot netat teter, a adud neisi teter genaim batauerdi =
Good (that) thou enter into life one foot, and bad (that) two feet enter into hell.

The Cambridge Anthropological Expedition to Torres Straits found in their psychological tests involving comparison-judgments, that the natives often expressed these in the form: "this big, that small," or "this heavy, that not heavy[5]."

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[1] For the Melanesian languages see Codrington's admirable treatise *Melanesian Languages*, Oxford, 1885, *passim*.

[2] R. H. Mathews, *loc. cit.* p. 262.

[3] Walter E. Roth, *Ethnological Studies among the North-West Central Queensland Aborigines*, Brisbane and London, 1897, p. 18. This is not an instance of pure opposition, as 'yet-cha' implies also gradation.

[4] Ray and Haddon, "A Study of the Languages of Torres Straits," *Proc. Royal Irish Acad.*, Part I. p. 585.

[5] Here again instances of gradation occur either independently of, or together with opposition. A higher degree of a quality would be referred to in Torres Straits as 'big.'

2. *The Method of Separation.*

We now pass to a method which in gesture-language appeared clearly to be of later date than the forms of expression first discussed and which the evidence of spoken languages proves to be a direct development out of the method of opposition. This is the method of *Separation*, the root-principle of which may be stated as follows: A quality is asserted of one of the objects of a pair, while the noun of the other object is governed by a preposition, postposition, verb, or adverb (or is put in a case) implying motion away from. Let the two objects be *A* and *B*, and the quality *x*, then the general type of such a comparison judgment reads as follows: Starting from *A*, *B* is *x*.

In its simplest form it is just that species of opposition which I called Exclusion complicated by the added notion of motion away from. What is asserted of *A* is denied by implication of *B*. Vestiges of this old denial are found even in the modern tongues of the Inflectional type. At the same time, even in very primitive languages the simplicity of the expression does less than justice to the complexity of the thought. The conception of gradation makes itself early felt. On to the old method of opposition there comes to be grafted a double assertion. *X* is predicated both of *A* and *B*, first by implication and then explicitly. For the simple positive form of the adjective there is substituted some form, different according to the type of language considered, implying gradation. The whole development is very gradual, and will be traced step by step in the instances now to be given.

Separation is exemplified in some tongues of the Incorporative type. Thus in Eskimo, the simple positive adjective is preceded by the ablative of the standard-noun:

tugtumit mikivok
(starting from) a reindeer, it is small = smaller than a reindeer.

Separation is (with opposition (*b*)) one of the two chief methods of expressing comparison in Stem-isolating languages. In the Malay-Dyak type, the noun of the standard in terms of which comparison is made is governed either (i) by a preposition signifying 'motion away from,' or (ii) by a verb meaning 'to pass from,' or 'to start from.' The ablative significance of this verb is sometimes emphasized by a separative preposition which may be combined with it.

Examples of (i) are the following:

Micronesia. *Caroline Is.* met kajalel jong meteu
thing beautiful from that.
„ *Marshall Is.* kom aurok jen ir
you good from them.

Polynesia. *Maori.* He mea pai atu i tena
a thing good 'thither from' that
„ *Futuna.* e lasi ake au ia Petelo
am tall 'hither' I 'from' Peter.

Melanesia[1]. *Mota.* o qoe we poa *nan* a gasuwo
a pig is big from a rat.
„ *Motlav.* no qo nilwo *den* naghow
a pig is big from a rat, &c., &c.

In Indonesia too, according to Ray, the method of Separation is quite common.

Thus *Malay*: besar rumah ini deri rumah itu
big house this from house that, &c.

This method is common to almost all the languages of the Melanesian group[2]. The only exceptions are to be found in tongues which are so very primitive as to be marked by an almost complete absence of prepositions. Thus the languages of the Santa Cruz[3] group, and of Savo[4], which have very few prepositions, are confined to the method of opposition. All other Melanesian languages fall into two classes, those in which the method of opposition survives side by side with the method of separation, while the latter is much more frequently used than the former, and those (a small minority) in which the method of opposition is more idiomatic and more usual than the method of separation. A detailed comparison of this family of languages in respect of their general development, their capacity to express abstract relations, the number of their numerals, and the differentiation of the parts of speech, would, if one had space enough at disposal, be specially valuable in proving that the method of Opposition is the most primitive expression of comparison, and that the method of Separation gradually develops, philologically and psychologically, out of the earlier form.

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[1] There is no doubt that, in most cases, the preposition used: *nan* (Mota), *den* (Motlav), *dean* (Volow), *en* (Vanna Lava), *ten* (Leon and Sasar), &c., &c.—are true prepositions of motion away from; thus

Motlav: lev nagrin *den* ke = take the club from him,

Mosin: tele o vanan ni me tŏ vatag men = this is the country he has already gone away from. Cf. Codrington, *passim*;

although, in some languages, in which the differentiation of prepositions has not proceeded so far, the same preposition may signify motion in any direction, 'away from' or 'to,' or even the locative 'at.'

[2] Codrington, *loc. cit.*, *passim*.

[3] Codrington, pp. 490, 496.

[4] Codrington, p. 560.

Examples of (ii) are the following:

Fiji: Sa uasivi vei koya e na vuku
excels from him in the wisdom = is wiser than he.

Florida. *Solomon Is.*: na vua te pile me sule vea ngaia
the alligator is big and large 'passing on' it = it is larger than an alligator.

Languages of the Catenary type also make use of the method of Separation, as one among others. Thus Coptic expresses comparison sometimes by means of a simple positive adjective, while the noun of the standard object is in the directive case[1]. But Coptic and allied languages also express comparison by means of a periphrasis corresponding to Latin *plus, plures,* which implies distinctly the notion of gradation. In various languages of Eastern Africa[2], which Misteli classifies as Catenary, and which according to some authorities represent a Semitic graft upon an indigenous stock, Separation and Gradation are combined in the same form of expression, the noun of the standard object being construed in the ablative, while the difference of degree is marked by modifying the adjective by means of words meaning 'very,' 'more,' or 'most.'

Languages of the Agglutinative type offer several instances of pure Separation. Thus all the tongues of the Finnish-Magyar group agree in having no special adjectival form to mark degrees of comparison. As a general rule, the comparative degree is expressed by means of the simple positive adjective, preceded by the noun of the standard object in the ablative. Likewise, the superlative degree is expressed by means of the simple positive adjective preceded by the ablative plural of the standards, which are either explicitly named, represented by pronouns, or replaced by the epithet 'all.'

Ostyak[3]: tau sâgarīwet kerĕs = the horse is great from the cow.
Samoyede[4]: the birch is tall, the pine is tall from this.
Tawgy: banda kolegita anie = the big one from all the fishes (*i.e.* the biggest fish).

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[1] This case expresses a number of relations, some of them requiring the genitive in Indo-Germanic, some requiring the ablative. In Coptic it is used after verbs of separation. Ziemer believes that when used in comparison it has a separative significance.

[2] Harari, Galla, Dankali, Somali. Cf. Fr. Müller: *Ueber die Harari Sprache im östlichen Afrika,* Wien, 1864, S. 7.

[3] M. Alexander Castrén: *Versuch einer ostjakischen Sprachlehre,* St Petersburg, 1858, §§ 72—78. See also, *Versuch einer jenissei-ostjak. und kottischen Sprachlehre,* § 88. The same holds good of Cottish.

[4] M. A. Castrén: *Gramm. der samojedischen Spr.*, St Petersburg, 1854, §§ 351—354.

The same general rule applies to the Tartar[1] and to the Mongol[2] languages.

The highly-developed tongues of the Dravidian group are not confined to any single method of expressing comparison. There is every reason to believe that one of the forms used is identical with the form just described. The simple positive adjective, in the nominative case, is preceded by the noun of the standard object, construed in a case which all native Tamil grammarians agree in regarding as the ablative of motion[3].

Inflectional or *Indo-Germanic type.* All the more primitive expressions of comparison in Indo-Germanic are founded upon the concept of Separation. Apart, however, from some very rare instances of pure Separation, which can be regarded as survivals of an earlier stage in the development of language, Separation is always combined in Indo-Germanic with Gradation. Ziemer has devoted to the proof of the general thesis above stated a work so complete and methodical that I need do no more than recall his chief arguments, and refer the reader for details to his *Vergleichende Syntax der indogermanischen Comparation*[4].

The chief items of evidence may be summarised as follows:

(i) The Gradation-forms of the adjectives in Indo-Germanic themselves imply the notion of transition and spatial separation, the gradation-suffixes possessing the same root elements as certain marks of spatial and quantitative relations. Ziemer accepts the theory of Scherer, according to whom the Comparative-suffix *-tara* is connected with a root *tar* = "to move over, or beyond"; and the suffix *-ra* is connected with a root *ar* = "to rise."

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[1] Castrén: *Versuch einer koibalischen und karaganischen Sprachlehre*, St Petersburg, 1857, §§ 41, 42.

[2] Castrén: *Versuch einer burjätischen Sprachlehre*, 1857, §§ 66, 67.

[3] Caldwell, Robert (*A Comparative Grammar of the Dravidian or South Indian Family of Languages*, pp. 179—80), argues that this case is not an ablative of motion, but a locative. Externally, there is nothing to distinguish the suffix of the ablative *-il*, from the suffix for the locative, and Caldwell is persuaded that, in spite of the traditional view of Tamil grammarians, "the Dravidian languages have no ablative properly so called, but only a variety of locative and instrumental suffixes, which are capable of becoming ablatives by the addition of appropriate verbs." This argument does not, however, suffice to prove that the case used in comparison has a locative meaning. Natives are unanimous in ascribing to it the meaning of an ablative of motion. Literally rendered in English it has the force of 'starting from,' as in the Finnish-Magyar family, and in a host of other languages I have already quoted from.

[4] Berlin, 1884. According to Ziemer, who starts with a psychological analysis of the act of comparison, to compare is not only to bring together, but to hold apart. Originally, it was this latter aspect which predominated. In the newer languages it has become subordinated to the former.

But the most conclusive evidence, and the most interesting to us because it falls directly into line with all we have had to say concerning languages of non-inflectional types, is to be found in an examination of:

(ii) The case-ending of the standard-noun[1], which in all Indo-Germanic languages almost invariably implies the concept of separation; and

(iii) The prepositions[2] used originally to reinforce the case-endings, and later as a substitute for them—and which again, with some exceptions, for a discussion of which I must refer to Ziemer, almost invariably imply the concept of separation.

3. *The Method of Apposition.*

Quite apart from the highest stages of the Syntax of Comparison, which are only met with in the Indo-Germanic languages, there remain in languages of widely different types certain forms which could not without violence be simply brought under the heading Opposition, or under the heading Separation. We have to review them now and account for them if possible. Thus in Chinese, the force of comparison is conveyed by means of a word signifying 'to compare.' For instance, in Canton dialect: ní pí thá kó = you compare, he high,—means "You are taller than he." So too in Shanghai dialect: pí ngú wau hau = compare I yet good,—means "he is better than I."

Various languages of the Catenary type use an expression which is very similar to that of the Chinese. Thus one of the two chief methods current in the Bantu family of languages[1] consists in using a locative, the meaning of which appears to be: 'with respect to.'

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[1] The ablative used in Sanscrit, Latin, and the Celtic languages; the genitive used in Greek, Old High German, and the Slav languages, are undoubtedly separative cases. Sanscrit also uses a genitive which Ziemer accounts for as either a separative by false analogy (the result of a transference of meaning from the ablative), or as a partitive (the result of a confusion between comparative and superlative). Sanscrit also uses the locative—a form which will be discussed presently; and the instrumental. The dative used by Gothic, Old Norse, Anglo-Saxon, Old and New High German, is regarded by Ziemer as a substitute for the Ablative of Separation.

[2] Classical Greek πρό and ἀντί merely emphasize the significance of the case-ending (genitive). Modern Greek ἀπό, though generally construed with the accusative, is the most emphatic expression of separation conceivable. Latin uses *a*, *ab*. Romance languages generally use *de*. The Slav preposition *od*, construed with the genitive, is the exact equivalent of Latin *ab*, Greek ἀπό. Irish and Gaelic sometimes use *do* to emphasize the Dative-Ablative.

e.g. Herero: O wami o mu-nene *p'ove*
I am old with respect to you.
Kaffir: Ndi mde *ku-we*
I am tall with respect to you.

Among Agglutinative languages, Hungarian forms a notable exception to what was enunciated above as the general rule. While in all the other languages of the Finnish-Magyar group the case in which the noun of the standard of comparison is put is undoubtedly an ablative of separation, Hungarian[2] uses a postposition -nál -nél, which has undoubtedly a locative significance. It is used whenever two objects are compared in respect of one quality, both for the comparative and for the superlative degree. It corresponds in every way to German *bei*, and is an answer to the question, where? An instance identical, I take it, with this is provided by languages of the Dravidian family. Thus Tamil makes use occasionally of an auxiliary verb in the infinitive governing the noun of the standard object in the accusative and signifying 'to see,' 'to show,' or 'to leave.' The phrase is concluded by the subject of the proposition, together with the adjective of the predicate in respect of which comparison is made.

e.g. Tamil: adei-(p)-pârkkilum idu nalladu
looking at that, this is good.
adei vida idu nalladu
quitting that, this is good.

Finally, to close the list with a brief reference to Indo-Germanic cases, Sanscrit sometimes employs the locative, instead of the ablative. Greek uses a variety of appositive prepositions (πρός, περί, ἐπί, ὑπέρ, παρά) governing the accusative; and the Germanic use of such strongly appositive prepositions as *vor*, *über*, *gegen*, is well-known.

I believe the various items of this class of forms to be based on a common underlying principle, which is a modification and development of the principle of Exclusion. This is quite obvious in Chinese, and there is next to no psychological difference between the form of statement in *Cinjandza*, "water and food, food good," and the form of statement used in *Canton* dialect, "you compare, he high." In either case, the two terms of comparison are explicitly brought together, and a predicate is asserted of the one to the exclusion of the other. The link between this primitive mode of expression and the Indo-Germanic locative or appositive accusative is to be found in the Finnish-Mágyar expression. Here the two terms are once more brought together, and a predicate asserted exclusively of one of them. The locative postposition which connects them deserves, however, a little discussion. Ziemer believes that such a locative can only be explained in one way. If it be said, as in Hungarian, "next to (or beside) water, wine is good," what the speaker means to lay stress upon, according to Ziemer, is not the spatial coexistence of the two terms of comparison, but on the contrary the experienced transition from the one to the other. For it would be absurd to suggest that they exert some mutual influence by the effect of their mere proximity[1]. The contrast, and my judgment of the contrast between them, is conditioned by a movement of attention from the one to the other. Thus the Finnish-Maygar locative in this case, according to Ziemer, implies the notion of transition; and he gives a similar interpretation of the Indo-Germanic analogue[2].

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[1] J. Torrend, *A Comparative Grammar of the South African Bantu Languages*, London, 1891, pp. 150 *seq.* The *Dinka* language also uses a locative particle *ke*, meaning 'with,' 'beside.'

[2] Cf. Ziemer, *loc. cit.*, S. 87.

But this fails to explain why the locative should ever have come to be employed at all. If both locative and ablative in comparison imply the same notion in the same way, namely the notion of transition, what internal distinction remains between these two cases? Why should they not in the absence of any such internal distinction be used indifferently? And why, finally, should the locative, in the great majority of tongues, exist merely by way of exception, whereas the separative case is almost universally used?

The following considerations may suggest an answer. In the first place, spatial coexistence, although quite irrelevant in the Hungarian instance of wine and water quoted above, undoubtedly may have a special significance when objects are compared, *e.g.* in respect of their size. Two unequal objects being placed one on the top of the other, part of one is immediately seen to project above or beyond the other. Placed side by side, the contrast between the long and the short, the broad and the narrow, is immediately striking. Again, where neither superposition nor exact juxtaposition is possible, it may be convenient to bring the objects close enough at all events to permit of rapid and repeated movements of the attention from the one to the other. In all such cases, what impressed the psychologist until quite recently, and what may presumably have impressed primitive man, is the spatial relation between the objects to be compared. Place one 'beside,' or 'over,' or 'opposite' the other, and it will appear to possess such and such a quality. In such circumstances, and from such a point of view, the locative is quite natural. Thanks to the actual coexistence of the two objects in space, simultaneous contrast brings out forcibly a particular quality (or a special degree of a quality) in one of them, which is asserted in the judgment. From the few cases in which spatial coexistence is possible, and its use perfectly natural; the locative gets extended to cases in which spatial coexistence is impossible or irrelevant. But the overwhelming majority of comparison-judgments concern objects which coexist neither in time nor in space. Juxtaposition and superposition are manœuvres which it requires a considerable degree of leisure to carry out. They are seldom performed in the bustle of everyday life, perhaps never performed except for the purpose of accurate measurement. For most of the purposes of practical life even in our present stage of development such nicety of discrimination is valueless. What interests us is to know, not the exact magnitude of the difference between one object and another, but whether there is a difference at all. The immediate impression of greater or less which in experiments on sensory discrimination is perhaps the chief factor in determining the judgment, is in practical life undoubtedly the chief factor in determining action. Now this immediate impression does not require for its arousal a simultaneity of two sense-impressions, even less does it require the simultaneous presence of a sense-impression and an image. Under certain conditions, it is stronger and more definite when the comparatum is separated from the standard by an interval of time, and when no vestige remains of an image of the standard. It is indeed conditioned by the transition of attention from the one object to the other. In Prof. James' words, there is first a consciousness of *m*, and then the consciousness of an '*n*-different-from-*m*.' The simplest form of the immediate impression of difference is accurately mirrored by the earliest method developed to express comparison, that of combined assertion and denial. To explain the growth of the method of Separation out of the method of Opposition, we have but to assume that together with the immediate impression conditioned by the transition of attention from standard to comparatum, man became aware of the transition of attention itself. As the great majority of our actions, so far as they are based on comparison at all, are determined by comparison of successively presented objects, it is readily intelligible that the principle of Separation, once developed, should have come to be generalized, and applied in most languages not only in cases where the objects of comparison are successive but also in cases where they are apparently simultaneous.

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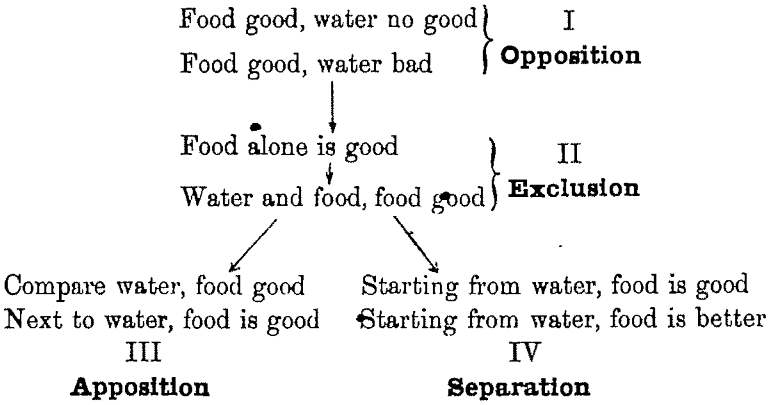
[1] *loc. cit.*, S. 134.

[2] *loc. cit.*, S. 37.

This explanation of the universality of the method of Separation and the relative insignificance of the method of Apposition might stand, even though, on analytic grounds, we were forced to accept the view that all comparison is comparison of the Simultaneous. On the other hand, the experimental evidence which contradicts this view appears to be immensely strengthened by the linguistic facts catalogued in this paper.

To sum up, the principle of Separation and the principle of Apposition are both offshoots of the principle of Opposition through that of Exclusion, and the development can be traced through a number of practically continuous stages as follows:



Food good, water no good
Food good, water bad
I **Opposition**

Food alone is good
Water and food, food good
II **Exclusion**

Compare water, food good
Next to water, food is good
III
Apposition

Starting from water, food is good
Starting from water, food is better
IV
Separation

4. *The Method of Gradation.*

I have said that the most primitive methods of expressing comparison-judgments (Opposition and Exclusion) are a perfectly accurate interpretation of the simplest experience in discrimination of successive sensations. In comparing two weights, or two shades of grey, presented one after another, the second presentation may often be accompanied by an immediate impression of 'heavy' or 'dark,' &c. The subject apprehends immediately the difference between 2 and 1: but it is the difference in its purity, and, spite of the logical paradox, only secondarily or not at all the duality of the terms, and their underlying identity, which occupy the focus of consciousness. The all-important feature of his immediate experience is perfectly expressed by the automatic cry: "that's a heavy one," &c. Properly speaking, there is no judgment of comparison here but the simple assertion of a quality. Of explicitly apprehended continuity between the presentations there is none. The concept of Gradation remains in abeyance. The laboratory subject's simplest judgment of sensory discrimination is exactly on a par with the comparison-judgment most usual among the Cherokees, the Abipones, the Damaras, the natives of Torres Straits or of Santa Cruz.

But as experience develops, so does my consciousness of the relativity of the immediate impression of difference. The same weight may appear to me light or heavy, the same colour bright or dark, under different circumstances. I begin to realise that the difference I apprehend is relative to a standard. From the "that's heavy, pure and simple" stage, my experience develops into the stage "that's heavy, coming after the other." The true comparison-judgment makes its appearance, in which there is an explicit reference of the difference to a *terminus a quo*. Already in this, the concept of *Gradation* is implied, but it unfolds itself and becomes explicit only by slow degrees. For gradation implies continuity, and simple sensory experience *semper facit saltum*. Its root-principle is that of contrast. We reach the 'more' and the 'less,' through a failure or conflict of contrast, by abstraction and generalization. The laboratory subject exclaims: "After *A*, *B* is heavy," remembers that *A* also gave him the impression of heaviness, reconsiders the quasi-automatic judgment, reflects on both experiences, and perhaps refers them to a common standard. The result of this complex process is a reflective predication of difference in degree. At this high level of development each quality comes to be conceived as forming a continuous scale of values, and the underlying identity, postulated by the logicians, between the terms of the comparison-judgment, becomes the object of explicit apprehension.

The development of linguistic forms in the race runs parallel to the pyschological development of the individual just described. Before we reach the Agglutinative tongues, hardly any instance can be found in which the concept of Gradation has stamped itself unmistakably on the Syntax of Comparison. Among Incorporative languages, Cherokee has a particle which is sometimes translated as 'more,' but which seems to be indistinguishable from an intensificative common in Amerind, and meaning literally 'strong.' The Stem-isolating languages[1], which represent nearly the lowest type at which Separation makes its appearance, yield a solitary instance of Gradation. The Indonesian group of tongues, which stands relatively high, "besides separating the compared objects by means of a preposition of 'motion away from,' modifies the epithet either (α) by means of a prefixed or affixed word meaning '*more*,' or (β) by means of an affix which has almost the force of a comparative inflexion[2]:

Examples of:

(α) *Madura-Java*: tingki = high, lebi tingki = more high.

(β) *Central Celebes*: madago = good, madago -pa -pai = better from.

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[1] Yet however inadequate their syntactical forms, there is no doubt in the opinion of McDougall, Rivers and other observers, that the natives of Torres Straits clearly possessed the notion of degree of quality, gradual change, identity in difference, &c.

[2] Mr Ray's communication.

Among the Agglutinative languages, while the more undeveloped, such as Samoyede and Tawgy[1], resort to diminutives and augmentatives, and to independent words signifying 'more,' 'much,' 'very,' 'entirely' as expressions for the comparative and superlative, the more developed have gone much further, either by differentiating forms of their own, or by borrowing forms from other languages. Thus Turkish[2] uses *dahi*, *daha* (*etiam*, *plus*) with the adjective; or else adds to the adjective the Persian forms *-ter* for the comparative and *-terin* for the superlative; whilst Finnish possesses affixes which differentiate the degrees of comparison, -mpa, -mpä, for the comparative, and -impa, -impä, for the superlative.

As is well known, the concept of degrees of comparison runs through the whole of the Indo-Germanic syntax of Comparison. Yet even in Indo-Germanic the explicit distinction of three degrees has not succeeded in extirpating every vestige of the more primitive mode of thought. The earliest apprehension of a difference was one not so much of degree as of kind.

Survivals of this earliest apprehension are to be found not only in the numerous pairs of contrary epithets which vulgar speech employs at every moment, but probably too in those cases in which there are no regular comparative forms. Thus good, better

bonus, melior, optimus
ἀγαθός, ἀμείνων, ἄριστος
&c. &c.

The difference of meaning in these cases appears to be rather one of kind than of degree; and this view is borne out by cases in which the same positive may have several alternative comparative forms, each of them with its own special shade of meaning; *e.g.* *ἀγαθός, ἀμείνων, βελτίων, λῴων, κρείττων*[3].

Finally, the occasional use of a comparative form in a positive sense, or of a positive form in a comparative sense appears to prove that even at a late stage of growth, Indo-Germanic can forget the distinction between difference in degree and difference in kind[4].

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[1] Castrén, *Sam. Spr.*, §§ 35, 358, 355.

[2] C. Wells, *Practical Grammar of Turkish.* Also Ziemer, *loc. cit.* S. 136.

[3] Cf. Wundt, *Völkerpsychologie*, Leipzig, 1900, Bd. I.; *Die Sprache*, Th. II. S. 15.

[4] Cf. Ziemer, *loc. cit.* S. 25, for instances.

IV. *Summary of the Evidence of Spoken Languages.*

The methods analysed in the preceding pages complete the list of those in respect of which Indo-Germanic can be compared with other language types. Separation, either in its pure form, or combined with Gradation, represents in all types of language except the Indo-Germanic, the highest level in the development of the Syntax of Comparison. But the more recent tongues of the Indo-Germanic type have advanced beyond Separation. The Romance, the Celtic, and the newer Germanic languages, while offering many curious instances of survivals of earlier methods, have in the main given up the use of the simple case or of separative particles to express comparison, and have adopted paratactic conjunctions. A detailed study of the method of *Composition* would have but a slender psychological interest. I do not propose, therefore, to say anything about it.

The evidence of the previous section may be summarised as follows:

1. The most primitive experience in sensory discrimination of successive objects is the apprehension of a novel feature in the second, together with the failure to apprehend such feature in the first, or the apprehension of its absence. The most primitive expression of the comparison-judgment in spoken languages consists in asserting a quality of the one object, while denying it of the other. Closely connected with this is the exclusive attribution of the quality to one of the objects. Practically coeval with these two forms is that which attributes contrary qualities to the two objects of comparison. At the lowest stage in the development of language these methods are found in exclusive operation (most of the Incorporative tongues, and some of the Catenary). At a later stage, they coexist with other methods which, in the course of further development, gradually acquire predominance.

In the Stem-isolating tongues Opposition and Separation are, roughly speaking, evenly balanced. In the Agglutinative, Opposition occurs by way of exception. In the Indo-Germanic, it survives as a vestige of the past. Not only from type to type can this continuous evolution be traced, but among the dialects of one and the same type. Thus among Stem-isolating tongues, the principle of Opposition is to be found in exclusive use in the most primitive dialects of the Melanesian group. In those dialects which have differentiated a greater store of prepositions Opposition is occasionally found in use, but Separation predominates. Finally, the more advanced Indonesian tongues hardly ever afford a true instance of opposition at all. A similar development could easily be traced in the Agglutinative and Indo-Germanic tongues.

2. Just as in the growing experience of the individual, the simple apprehension of a novel feature in *B* passes over by countless degrees too subtle to describe into the reference of this apprehended novelty to a standard, so too in the linguistic development of the race the old method of Exclusion becomes gradually superseded by forms in which an explicit reference is made to the standard of comparison by connecting it organically with the comparatum. As already seen, from the stage: "Food only is good," we reach, through a stage in which the objects compared are joined by a mere copulative "Water and food, food good," two divergent methods, Apposition and Separation, the latter of which for the reasons set forth above attains to a very marked predominance.

3. The reference to a standard is already an incipient recognition of qualitative continuity. In the growth of language, unmistakable instances of the principle of Gradation first make their appearance at the stage when Separation is already well developed. A number of stages are here again to be distinguished, according as pure Separation predominates, or as Separation and Gradation are methods of equal importance, or as Gradation predominates over Separation, the meaning of the old separative forms tends to become obscured, and the forms themselves tend to survive merely as vestiges of a state of things long passed. Finally, in the newest tongues of Indo-Germanic type, while the Gradation-forms of the adjective are the all-important sign of comparison, the reference of comparatum to standard is generally effected no longer by a locative or an ablative case, no longer by an appositive or a separative preposition, but by a paratactic conjunction. The propositional form of comparison-judgments with which we are familiar, and upon the analysis of which the older psychological doctrines of comparison are chiefly founded, is thus seen to be the final stage in a complex and lengthy process of evolution.

A COMPARISON OF SOME MENTAL AND PHYSICAL TESTS IN THEIR APPLICATION TO EPILEPTIC AND TO NORMAL SUBJECTS.

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The aims of the investigation. The subjects examined in the course of the work: one normal group and two epileptic groups. The mental and physical tests and the methods employed. Results of the application of the tests. In recognition, immediate memory, and reactions there are in general marked differences between the groups. In sensory discrimination, rapidity of voluntary movement, rhythmic movement, and maximal voluntary contractions—processes which are relatively simple—the differences of the groups are slight. The effects of practice.

It is the aim of the following paper to compare several experimental methods which may be used in investigating the condition of individuals suffering from mental disease, and to present the results which have been obtained by applying the methods to epileptic and to normal subjects[1]. One has to take into account in work with abnormal subjects not merely the intelligence and education, but also the likes and dislikes of the patient, and it is only by trial that one can discover what modifications of method are practically suitable. It was also intended when the work was planned to employ the methods in determining the nature of some of the changes which are brought about by the epileptic fit, and thus to contrast in individual patients the alternating phases of comparatively normal behaviour and behaviour as influenced by the fit.

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[1] The chief points in the investigation were brought before the Psychological Society, July 25, 1903.

It was hoped in this way to avoid some of the difficulties which beset the comparison of a group of insane subjects with a group of normal subjects[1]. It was found in the end that the practical difficulties of securing an adequate number of observations made it impossible to carry out this project, and therefore the main body of results which are presented here deal with the relations between normal subjects and epileptic patients observed in the intervals between fits. One of these difficulties may be mentioned. A number of patients who had been brought over to the laboratory several times refused to come again, having formed the delusion that electricity was put into them by the apparatus employed in some of the experiments.

The work was carried on in the Pathological Laboratory of the London County Asylums, Claybury, Essex, during the years 1902 and 1903. I am greatly indebted to Dr F. W. Mott, Director of the Laboratory, for the opportunity of carrying on the work and for his constant assistance, and also to Dr R. Jones, Superintendent of the Asylum, for permission to examine the patients. During the investigation 23 male patients were examined, but it was finally decided to use the results from the 10 patients who afforded the most numerous and reliable results. Two minor groups were formed, each including 5 individuals. In the first group the patients were comparatively rational in conversation and behaviour during the intervals between fits; in the second there was, on the whole, marked dementia. The average age was in the first group about 35, in the second about 38 years. With these patients I have been able to compare a normal group of 6 subjects (men), about 26 years of age on the average. All, except one, were attendants in the asylum and may be regarded as belonging to the same social class as the patients. Observations were secured from the patients on 5 days, these days coming at irregular intervals. Five of the normal subjects contributed results on 3 days, one on 2 days. I regret that it was not possible to secure more numerous experiments in the normal group. It may, however, be assumed that a normal subject will tend to give more constant results, and that in this way the disparity in number of experiments between normal and epileptic groups may be of minor importance. Data will be presented later on by which some idea of the variations in the different groups may be gained. The tests which were applied dealt with the following subjects:—recognition; immediate memory; sensory discrimination; reactions involving movement and choice; rapidity of simple voluntary movement; tremor; rhythmic movement; maximal voluntary contractions. The object of these tests was to secure some indication not merely of sensory and ideational changes but of alterations in the processes of motor innervation.

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[1] This principle has been already applied in investigating epileptic subjects by A. Gross, *Verhalten einfacher psychischer Reactionen in epileptischen Verstimmungen*, *Psychologische Arbeiten*, Leipzig, 1901, Bd. III. S. 385.

The following brief notes, for many details of which I am indebted to Dr Pugh, Medical Officer in Claybury Asylum, give some particulars regarding the patients whose results are embodied in the tables. All the patients have fits of the *grand mal* type.

GROUP I. **Comparatively normal patients.**

A. Aged 20; gardener; admitted Aug., 1900; duration of fits before admission 9 years; over 7 fits per month; quiet and rational in manner and talk during the times when he is in the laboratory; immediately after fits intractable and impulsive.

B. Aged 24; worker in piano factory; adm. Jan., 1902; duration 3 years; several severe fits since admission; emotional and very ready to talk; after fits excited and impulsive.

C. Aged 41; packing-case maker; adm. June, 1899; duration 8 years; about 8 fits per month; gives intelligent and graphic account of illness and history; after fits violent and troublesome.

D. Aged 35; clerk; adm. Oct., 1893; duration 6 years; over 5 fits per month; intelligent, but rather simple; after fits violent and abusive and has auditory hallucinations.

E. Aged 56; waiter; adm. Sept., 1900; duration of fits probably from boyhood; fits infrequent; intelligent but moody; after fits restless and developes auditory hallucinations.

GROUP II. **Patients showing marked dementia.**

F. Aged 31; shoemaker; adm. Sept., 1893; duration 7 years; fits infrequent; dull and unintelligent; after fits impulsive and aggressive.

G. Aged 48; soldier; adm. Dec., 1897; duration 8 years; fits frequent; talkative and rambling; after fits pugnacious and intractable.

H. Aged 47; belonged to band of travelling circus; adm. May, 1895; duration 10 years; about 8 fits per month; rambling and occasionally incoherent, with confused religious ideas; after fits electrical delusions.

I. Aged 44; messenger; adm. March, 1895; duration 25 years; fits frequent; rambling and occasionally incoherent, very cheerful; after fits troublesome and pugnacious.

J. Aged 21; shoemaker; adm. Sept., 1893; duration 2 years; about 5 fits per month; simple and feeble minded; recovery from mental confusion after fits gradual.

Methods.

Recognition. The experiments on recognition, with which the experiments were begun each day, were carried out by means of series of pictures and words. The pictures were reproductions of the paintings of great artists, which were printed on cards of the size of ordinary playing-cards: the words were typewritten on a slip of paper. A group of 7 pictures was selected and handed to the patient as soon as he had seated himself at the table, with the request that he would look at them so as to be able to know them when they were shown again later on. After he had looked at the pictures as long as he pleased a series of 7 words relating to ordinary objects was given him and he was asked to read it aloud; he was again told to be ready to recognise the words later on. At the end of the hour the group of 7 pictures mixed up with 7 new ones was handed to him and he was asked to pick out the ones he had seen before. In the same way a slip of paper was given him bearing 7 new and 7 old words mixed up in 2 lines, and he was asked to say which words he had seen before. On one or two occasions when the words were inattentively or imperfectly read I asked the patient to read a second time, or when the recognition was not properly carried out I read the words aloud one by one, asking after each whether it had been seen before. By having an equal number of old and new pictures and words one can get a fairly precise idea of the nature and extent of the mental weakness. The patient may forget all the 7 pictures and words or part of them: he may completely confuse old and new and imagine he has seen all before, or the confusion may extend only to part of the pictures and words. Inasmuch as we have a predominantly visual process in the recognition of pictures while in the recognition of words we have probably a highly complex combination of visual, auditory, and motor processes we have the means of analysing in some detail the character of the changes in recognition.

Immediate memory. This process was tested by reading aloud to the subject a series of letters arranged at random which he was asked to reproduce the moment the reading was finished. The rate of reading was the same for all, and was regulated by a metronome beating at the rate of 108 per minute. The procedure was substantially the same as that employed in a former investigation on "The Range of Immediate Association and Memory[1]," the only difference being that whereas previously the series which were presented to the subjects contained 4, 5, 6 up to 10 letters, in this instance it was decided to omit the series containing 8 and 10 letters. The reason for this omission lay in the circumstance that as this test was only one of a number it was advisable to make it shorter and easier than on the former occasion when it was studied in detail. The series containing 4 letters (which we may call stage 4) was given first; the series containing 5 letters (stage 5) followed and so on. As a rule there were not more than three experiments at each stage on one day. By means of this test combined with that for recognition we can compare the memory for immediately preceding events with the memory for events occurring at a time further removed. As soon as these experiments were finished a pause was made to relieve the strain on the patient. He was encouraged to smoke and talk about the things that interested him, his health, his history, and so on.

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[1] *Archives of Neurology*, Vol. II. p. 767. This paper contains also some preliminary observations gained with the first test, viz., recognition.

Reactions. After this interval the reaction experiments were taken[1]. A pack of playing-cards was divided into halves, one half being used for choice experiments, the other to determine the speed of the movements by which the cards were handled. In the choice experiments one card from each suit was laid in front of the patient and he was asked to distribute the rest of the cards into 4 heaps, each containing the cards of one suit. In the other set of experiments the patient was asked to throw the cards one by one on the table so as to form one heap. Two experiments were made each day with choice and with throwing: in both cases the time was taken by a stop-watch giving fifths of a second. The reason for using this method for reaction experiments lay in the wish to secure activities which would be more or less familiar to the subject and would possess some interest. The difficulty in that case is that it is hard to find an activity which is not strange or difficult and yet is not influenced by the previous habits of the individual. The disadvantages which arise from the fact that some persons are much more familiar with cards than others will be dealt with later on.

Sensory discrimination. The test for sensory discrimination, which was taken next, was carried out by means of lines drawn in black ink on sheets of paper, measuring 17 by 12·5 cm. One sheet, with a line 100 mm. in length, the standard line, was placed in front of the patient towards the left. On the right were placed in succession 15 lines

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[1] This method of studying reaction times by playing-cards was introduced by Jastrow, *Science*, Vol. VIII. p. 237. It need hardly be remarked that the term 'choice' as used here is employed only for convenience and that more is involved than mere decision between alternatives.

('compared lines') one of which was equal to the standard line while the others differed from it by 1, 2, 3, 4, 5, 6, 8 mm., the number of lines that were longer than the standard line being equal to the number that were shorter. The patient was asked in each case to tell whether the compared line was equal to the standard line, or greater, or smaller than it. On the first few days the sheets were laid on the table in front of the subject; afterwards they were placed on a small stand which held them at an angle of about 30° above the table. The observations made in these two ways have been taken together; the patients were allowed to look at the line in any way and for any length of time they pleased so that the difference of results due to the position of the lines may be regarded as negligible. The whole set of lines was presented twice each day; the order in which the lines came being determined by chance and being changed each time they were presented. It was suggested to the patients that they should regard this as a test for eyesight; they were in all cases encouraged by being told that they had done extremely well, and that a second trial might remove any small errors they had made.

Rapidity of movement, tremor and rhythm. The rapidity of voluntary movement was determined by finding the rate at which the patient could tap on a key which was connected with an electric signal writing on the recording surface; the problem before the patient was easy and simple, while the motive of seeing how quickly he could carry on the movement was one which might be expected to appeal to him. Owing to the fact that the Jacquet chronoscope was available for securing accurate records only in the later stages of the work, the results for tapping and for the following experiments on tremor are based on one day's work only. The rate of tremor was determined by getting the patient to press gently with the forefinger of his right hand on the button of a Marey cardiograph, which was connected with a delicate Chauveau tambour. The forearm was first rested on a box which brought hand and arm to the same level; after the observation had been continued in this way for about half-a-minute the box was removed and the tracing taken for some time longer. The object of this alteration was to separate in some measure the movements of the hand and of the arm. Rhythmic movement was tested by asking the patient to keep time to the beating of a metronome which was made to beat first at the rate of 60 beats per minute, then at double this rate. The patient kept time by pressing on a piece of rubber tubing lying before him on the table: one end of the tubing was closed, while the other was joined to the tambour. It was hoped to get indications not merely of the accuracy with which time was kept, but also of the character of movements made in carrying out the rhythm. The apparatus was so arranged that the patient sat in one position and carried through the different movements without further trouble than that of moving to the table on which the apparatus was placed.

Maximal contractions. These observations on rapidity of movement, on tremor, and on rhythm were carried out together each day in the earlier stage of work. At a later stage this group of experiments was replaced by the work on maximal voluntary contractions. For this purpose a modification of Mosso's ergograph was used. In place of the weight attached by a string passing over a pulley to the moving carriage which bears the writing lever there was introduced a spring balance of such a strength that it could not be pulled out to its full extent. In place of the arm-rest with the attachment for the finger a piece of apparatus was arranged by which the force exerted in the grip of the hand could be registered. In this arrangement the hand grasped a fixed vertical pillar and a moveable vertical bar connected by a horizontal arm with the moving carriage: the grip of the hand bore directly in a straight line on the spring balance. There are several advantages in this form of ergograph. Allowing the recording surface to move at a moderately fast rate we get a tracing of the full extent of the contraction of the muscles of the hand. This was of importance, inasmuch as the aim was not to secure a long series of contractions showing fatigue, but to get an analysis of a few maximal contractions. Further, one may expect in this way to secure the willing cooperation of the patient more readily than if he is asked to carry out a prolonged and fatiguing series of contractions. He was always asked to show how strong he was, the appeal being thus made to motives whose presence might be relied on. The signal for the patient to exert himself was the sound of an electric bell so arranged that when the bell was struck by the hammer another circuit passing through the hammer and the bell to the electric signal was closed and the moment of stimulation registered. The patient was requested to grip as hard as he could, and then let go as soon as he had done his best.

Results.

In the majority of the tables which follow I have presented the results of the normal and the two epileptic groups in successive horizontal lines, thus facilitating comparison. It has not seemed advisable to lengthen the paper by presenting and discussing individual results and therefore the numerical data have been condensed as far as possible. At the same time the central values and the mean variations are presented in many cases in addition to the averages, so that a judgment can be formed as to the reliability of the averages. The central value is specially important in the treatment of short series of experiments, such as one has often to be content with in pathological work, for it gives us an objective means of minimising the effect of exceptional and disturbing values. It has, however, been used merely as a supplement, and the principal stress has been laid on the arithmetical mean or average, which represents better the mass of the results.

Recognition. In the following table the results for pictures and words appear separately. Under each of these headings are given two classes of error; (*a*) those in which the objects originally presented are not recognised, the error being termed one of *forgetting*, (*b*) those in which the new objects are thought to have been seen before, the error being one of *confusion* between old and new. In each case the average for each class of error has been first calculated for the individual; these individual averages form the basis of the general averages presented in the table. In the same way the mean variation (*m.v.*) for each error has been calculated first for the individual, and then, on the basis of this, for the group. The total number of experiments on which the general averages are based is 25.

TABLE I. *Recognition.*

	Pictures				Words			
	Forgetting		Confusion		Forgetting		Confusion	
Groups	av.	m. v.	av.	m. v.	av.	m. v.	av.	m. v.
Normal	1·2	0·5	0·8	0·4	1·7	0·6	0·8	0·5
I. Epileptic	1·3	0·7	0·7	0·5	2·1	0·8	1·9	1·3
II. Epileptic	1·7	1·0	2·5	1·2	2:2	1·2	3·3	1·3

In almost all the divisions of the table it is evident that there is much greater difficulty in recognising words than in recognising pictures. At the same time the relations between the groups are similar in both. From this we may conclude that the relations between normal and pathological groups are the same whether the processes in recognition are exercised on pictures or on words. And the fact that the relations are the same may be taken as evidence that the failure to recognise in the second epileptic group is not due to inability to read perfectly. With one exception there is an increase in the number of errors in each column as we pass from the normal to the first epileptic group and then to the second. It will be observed however that the increase is much more marked in confusion than in forgetting. In fact the preponderance of forgetting over confusion which holds in the normal and the first epileptic groups is changed to the opposite in the case of the second epileptic group. This fact has an interesting bearing on our experience of ordinary life. Everyone forgets many things from day to day, while errors of confusion occur relatively seldom and are much more noticeable when they do occur.

As it was not practicable to give more than one group of pictures and of words each day the mean variation in this test is of less value than in the other tests where more than one experiment was made on each day, and it is of course less in the normal group than in the epileptic groups. But taking the figures as our only means of comparing the groups in regard to constancy of results, we see that there is in every instance an increase of the mean variation as we pass from the first to the last group. If we take the central value for each individual, thus avoiding some quite exceptional results, and calculate the average of these values, we find that while the numbers differ slightly from those in Table I., being in every case somewhat lower, the relations of the groups remain the same in regard to each of the points mentioned above.

Immediate Memory. The first of the two tables which follow contains the results of the normal group, while the second contains the combined results of the two epileptic groups. The average number of correctly reproduced letters for all stages taken together was found to be nearly the same in the two epileptic groups, viz., 18·7 in the first and 18·0 in the second, while it was markedly different from the normal total 23·1. This being so it seemed best to treat the results of the two epileptic groups as being on the same level, and thus reduce the number of tables. For a detailed account of the way in which the valuation of the results is effected reference must be made to the paper already mentioned on "The Range of Immediate Association and Memory." It will be sufficient here to give the following brief explanation of the descriptive terms used in the tables.

(1) *Rightly placed* denotes letters which are reproduced correctly in every respect: (2*a*) *group transposed* refers to letters in a group whose position has been changed, *e.g.*, with a series *a b c d e f g* we might have *a b c* reproduced at the end; (2*b*) *inversion in right position* refers to letters in a group which mutually change places while the group as a whole retains its right place; (2*c*) *inversion in wrong position* refers to letters which are inverted in a group which does not retain its right place; *e.g.*, with the above series we might have *b a* reproduced at the middle of the series; (3) *wrongly placed* denotes letters which were in the original series, but retain no trace of their original position; (4) *omission* refers to letters of the original series which are not reproduced; (5) *insertion* refers to reproduction of letters which were not in the original series; (6) *repetition* refers to the repetition of letters already reproduced; (7) *defect* indicates the number of letters by which the reproduced series, whether right or wrong, on the average falls short of the correct number; (8) *excess* indicates the number by which the reproduced series exceeds the right number. At the head of each column is given in Roman numerals the number of letters presented at each stage. In all cases the values are obtained by taking the arithmetical mean of the total number of letters coming under each heading. The number of experiments on which the individual averages are based differs slightly in the different subjects; in general the average for the group is based on 75 experiments.

TABLE II. *Immediate Memory: Normal Group.*

Stage	IV	V	VI	VII	IX
1. Rightly placed	3·9	4·8	4·9	4·9	4·6
2 *a*. Group transposed	—	—	—	0·2	0·5
2 *b*. Inversion in right position	0·1	0·1	0·3	0·4	0·3
2 *c*. Inversion in wrong position	—	—	0·1	0·1	0·2
3. Wrongly placed	—	—	0·1	0·4	0·8
4. Omission	—	0·1	0·6	1·0	2·6
5. Insertion	—	0·1	0·4	0·6	0·8
6. Repetition	—	—	0·1	0·1	0·4
7. Defect	—	—	0·1	0·3	1·4
8. Excess	—	—	0·1	0·1	0·1

TABLE III. *Immediate Memory: Epileptic Groups.*

Stage	IV	V	VI	VII	IX
1. Rightly placed	3·6	4·1	3·8	3·8	3·0
2 *a*. Group transposed	—	0·1	0·2	0·2	0·4
2 *b*. Inversion in right position	0·1	0·1	0·4	0·2	0·3
2 *c*. Inversion in wrong position	—	—	—	0·1	0·2
3. Wrongly placed	—	0·1	0·3	0·6	1·3
4. Omission	0·2	0·6	1·3	2·0	3·8
5. Insertion	0·3	0·5	0·9	1·2	1·6
6. Repetition	—	0·1	0·3	0·4	0·7
7. Defect	—	0·1	0·3	0·6	1·6
8. Excess	0·1	0·1	0·2	0·2	0·1

We have in this test fortunately a standard with which we can compare the results of these two tables. If we take the normal results obtained in the former research from 11 individuals, none of whom is included in the present group, and determine the number of rightly placed letters at all stages taken together, we find that the total is 23·2, a number practically identical with that of Table II, viz., 23·1. Again, the similar totals in the two groups which showed decided evidence of pathological weakening were 18·4 and 18·3, while the totals in the two epileptic groups are 18·7 and 18·0. In the light of these correspondences the similarity of the results in the two epileptic groups becomes less surprising: it means that both are markedly affected and touch a low pathological level. The lack of correspondence between this test and that of recognition in the case of the epileptic groups is evidently another expression of the fact which is so remarkable in individual cases, that immediate memory may be practically normal while memory for the ordinary experiences of each day is completely lost, and conversely ordinary memory may be retained while immediate memory is deeply affected. We may say in other words that the processes involved in the two tests do not run parallel, but are independent variables. When the central values for each stage in each individual's results are determined, and the averages taken, the figures for the groups in order are 24·1, 19·5, 19·3; *i.e.*, the ratios are the same while the absolute values are somewhat higher.

When we examine the various classes of error in the two tables the pathological character of the epileptic results is confirmed. These results show a decided increase in the classes of wrongly placed letters, of omission, insertion, repetition, and a slighter increase in defect and excess: in other words, an increased associational disorder is shown by all these classes of error. On the other hand the classes 2*a*, 2*b*, 2*c*, which include all the letters that retain some trace of their former position, *i.e.*, indicate partial disorder, are not markedly different in the two tables. This, again, confirms what was found in the former research that the evidence of pathological change is to be found not in errors indicating partial disorder but in those indicating relatively complete disorder.

That the difference between the epileptic groups though slight is not to be entirely neglected seems clear from the fact that it shows itself regularly in the various classes of error. The relations of the groups can be brought out most clearly by the following arrangement, in which the errors at all stages are added together:

	Wrongly placed	Omission	Insertion	Repetition	Defect	Excess
Group I.	2·1	7·5	2·8	1·0	3·9	0·3
Group II.	2·5	8·3	6·1	1·9	1·2	1·0

We see here that the increase of error is in the same direction at each point except one: the diminution of defect in the second group is accounted for by the increase in omission, insertion, and repetition.

We found in the first test that there was a marked increase of errors of confusion in the second epileptic group. The class of error in this test which corresponds to confusion is that of insertion. It is interesting to observe that the errors of insertion in the second group are more than double what they are in the first epileptic group: we have in this fact a valuable confirmation of some of the results in the first group. The anomalous result in the first test that errors of confusion in pictures were slightly fewer in the first epileptic than in the normal group is not supported by the present test: the errors of insertion at all stages in the normal group amount to 1·9 as against 2·8 in the first epileptic group[1].

A report on the patients' memory for time may be introduced here. During the latter part of the investigation they were asked how long it was since they had been in the laboratory before and how long their stay in the asylum had been. It is extremely difficult to estimate the value of the answers when they are sometimes given in vague statements such as "about a month" and sometimes are accurate to a day. In general, however, it seemed clear that the members of the first epileptic group had a better understanding and memory of time than those in the second group.

Sensory discrimination. There can be little doubt that the mental processes in sensory discrimination are simpler and demand less mental capacity than those involved in the preceding tests. On the ground that in mental disease it is the more complex and highly evolved activities which are most liable to suffer injury, we might expect that this test would show less difference between the three groups than do the others. This is in fact the result brought out in the following table.

TABLE IV. *Sensory Discrimination.*

Groups	Correct judgments	m. v.	'Longer'	'Equal'	'Shorter'
Normal	9·7	1·3	4·0	5·6	5·4
I. Epileptic	9·2	1·8	3·5	6·2	5·8
II. Epileptic	10·4	1·2	6·0	4·1	4·9

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[1] It may be mentioned here that the hearing of the patients was tested and was found to be fairly good in all except one, who is included in the first epileptic group.

The columns headed 'longer,' 'equal,' 'shorter' contain the average number of judgments of each kind made by the subjects. The proportion of correct judgments is seen at once when it is remembered that the possible total of such judgments was 15: of these 1 should have been 'equal,' while 7 should have been 'longer' and 7 'shorter.' As there were two judgments each day on each of the compared lines, the total number of experiments on which the general average is based is in each epileptic group 50.

The differences between the averages of correct judgments in the three groups are slight: if we lay any stress on them we must say that the second epileptic group is the more accurate, this being indicated not merely by the average but by the mean variation. This general result is confirmed by the fact that perhaps the most demented patient in the second group gave a higher average of correct judgments, viz., 12·9, than any other subject normal or abnormal, and that his mean variation was also the smallest in the three groups. The patient who gave the next highest value—11·4—was also very demented. We have in this test evidently a mental activity which is in no way affected by the mental enfeeblement that is evident in other respects. Here, again, we can employ the central value to discount the exceptional values; taking the average of the individual central values we find that the numbers do not differ appreciably from those based exclusively on the arithmetical mean, being for the successive groups 9·7, 9·1, 10·5. The variations in the averages of judgments in the last three columns do not appear to possess any significance.

Leaving aside the question of correlation with other tests we may attempt to estimate the factors which enter into the judgments. If we divide the subjects, normal and abnormal, into a higher and a lower group, one giving the larger number of correct judgments while the other gives the smaller, we find the relations to be those embodied in the following table.

TABLE V.

Group	Correct judgments	m. v.	'Longer'	'Equal'	'Shorter
Higher	11·5	1·1	5·4	3·4	6·2
Lower	8·0	1·5	3·5	7·2	4·3

It is to be noted that the smaller mean variation goes with the more correct judgments: in other words, the more correct perceptions are also the more constant. In both groups there is a tendency to underestimation of the compared lines shown in the excess of judgments 'shorter.' The factor which differentiates the groups in this relation is the greatly increased number of judgments 'equal' in the lower group; *i.e.* the failure is due not to a positive tendency to error in one direction or the other, but to inability to discriminate.

It might be supposed that the results of these tables would be distinctly modified by imperfect vision on the part of the subject. In view of this possibility all the subjects were examined with Snellen's test types. Two patients were found whose vision was unusually good: their correct judgments numbered 11·3 and 8·4; two were found with very defective vision; their averages of correct judgments were 10·8 and 9·6. In view of these facts it seems clear that the character of the results is not due in any appreciable degree to peculiarities of vision. It is quite conceivable that an individual whose vision is poor may on that account unconsciously take greater care in discriminating what is before him and so compensate for his defect.

Reactions. In the following table we have the time in seconds which is taken by the three groups, (*a*) to distribute cards into four heaps according to their suit, (*b*) to throw the same number of cards in succession on one heap.

TABLE VI. *Reactions.*

	Choice		Throwing	
Groups	av.	m. v.	av.	m. v.
Normal	17·0	1·6	8·3	0·6
I. Epileptic	23·6	2·7	11·0	1·0
II. Epileptic	32·0	3·0	13·6	1·2

It will be observed that there is no exception to the rule that as we pass from the normal to the first and then to the second epileptic group the length of time required and the mean variation distinctly increase. While the difference between choice and throwing times, which may be called pure choice time, is 8·7 in the normal group, it is 12·6 in the first and 18·4 in the second epileptic group. That these values are not unduly modified by exceptionally long reaction times may be concluded from the fact that the averages of the central values for choice and throwing are practically the same as the averages of the arithmetical means, being in the three groups respectively 16·8, 23·7, 32·0 for choice, and 8·2, 10·7, 13·4 for throwing.

Before we can draw conclusions from these figures we have to inquire how far the results are influenced by the previous habits of the subjects. All the normal subjects have played some kind of card game. When we arrange the patients into two sets according to the speed with which the throwing was carried out we find that the three patients who said they had never played cards fell into the set which takes the longer times. This fact indicates that previous habits have influenced facility of movement. On the other hand, when they are arranged according to pure choice times, two of these three are in the quicker set, and one is almost exactly intermediate in value between the two sets. This would indicate that previous familiarity with cards was of little importance in regard to choice. As a fact two out of these three patients are included in the first epileptic group so that the quicker reaction in this group can hardly be due to greater familiarity with cards. We can finally disregard the results of these three patients and combine the rest of the values for the epileptics into one set of averages. When this is done it is found that the average choice time is 28·5 sec., and the throwing time is 11·5 sec., while the difference is 17·0, *i.e.* about double what it is in the normal group. It must be remembered that these reaction tests were arranged specially with the view of comparing the condition of the patients before and after fits. But even on the less satisfactory basis of a comparison of groups we have good ground for putting down the greater part of the retardation in choice and in throwing to the same causes which produce the differences in the other tables.

In the course of the choice experiments errors were sometimes made by the subjects, the cards being put on the wrong heaps. Sometimes the error was unnoticed: sometimes it was observed and corrected. In order to determine whether the errors unduly influenced the averages I have worked out revised figures for the experiments which included no errors. The revised values for the three successive groups are 16·1, 22·2, 31·5. It is clear from this fact that no substantial error was introduced by this factor into the averages of the table. There is no marked difference between the groups in regard to the number of errors. That the numbers are slightly greater in the normal group may be due to hurry and carelessness[1].

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[1] In taking the central values into consideration above I have given the averages of all the individual central values. We can however carry out the central values more thoroughly by rejecting the extreme individual values and taking the average of the remaining values – 3 in the epileptic groups and 2 in the normal group. When this is done the following relations are brought out. In recognition (taking errors of forgetting and confusion together) the errors for the three groups in order are for pictures 1·5, 1·7, 3·7, and for words 2·0, 3·0, 4·0. In immediate memory the values of rightly placed

Rapidity of movement and tremor. We have in the repetition of an elementary muscular activity, such as is involved in tapping a key, a test which, like visual discrimination, requires a relatively simple psychophysical activity. We might, therefore, expect that it also would bring out relatively slight differences between the groups. And when we look at the results we find that there is practically no difference between the groups. The rate of tapping in the first 8 seconds is 6·3 per sec. in the normal group and 6·2 in the first and second epileptic groups: the rate in the second 8 seconds is 5·9 in the normal group and 6·3 in the two epileptic groups. These values are of interest when we compare them with the averages for throwing cards: the form of movement involved in placing cards, as we saw, brought out distinct differences in the three groups. The conclusion seems to be justified that both in the motor and the sensory spheres it is the more complex processes which show themselves to be most affected. Compared with tapping, the coordinated contractions of the muscles in both hands required in holding and in placing the cards form a much more complex process[1]. In regard to nearly all the graphic records of tapping, normal and abnormal, one is struck by the regularity in the rate of succession of the movements. Occasionally there is a straight line traced by the electric signal where two or three indications of movement should have been found, such gaps being found both in the normal and the abnormal records.

The chief interest of the results which I am able to present in regard to tremor lies in their correlation with the data for voluntary movement. It has been asserted by Levy-Dorn[2] on the basis of observations on normal individuals and on persons suffering from nervous disease that there is in general a close agreement in the number of voluntary contractions per second and the rate of tremor. He has suggested also that where the tremor rate of a patient could not be determined it could be inferred from the rate of voluntary movement. In the following table the epileptic subjects have been divided into two sets, according to the speed with which the voluntary

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letters are 24·0, 19·5, 19·7. In sensory discrimination the correct judgments number 10·5, 8·8, 10·7. In choice the figures are 15·7, 24·0, 29·8, in throwing they are 7·3, 10·8, 13·0. It will be observed that there are only slight differences between these values and the averages already given and that in general the relations remain the same.

[1] Cf. Jack, "On the analysis of voluntary muscular movements," *Proc. Roy. Soc.* 1895, Vol. LVII. p. 477.

[2] *Neurologisches Centralblatt*, 1898, S. 522. A communication in regard to the relations of voluntary movement and tremor was made to the Physiological Society, Jan. 17, 1903.

movements are carried out, the number of patients in each group being six in this instance.

TABLE VII.

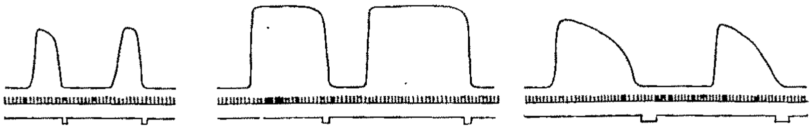
	Voluntary movement		Tremor
	First 8 sec.	Second 8 sec.	15 sec.
More rapid	7·0	7·1	9·5
Less rapid	4·9	5·4	8·5

It is clear that if there were such a correspondence as has been maintained it ought to show itself not merely in the results for all the patients taken together, but for the two sets into which they have divided. The average rate of tapping for these 12 patients is 6·2 per sec.: the rate of tremor is 8·95 per sec. The same want of correspondence is shown in the detailed treatment of the table given above. So far as epileptics are concerned there is no ground for assuming an identity in the rates of the two processes. It is, however, clear from the table that, while there is no identity, there is a certain correlation of rates in so far as there corresponds to the less rapid rate of movement a less rapid rate of tremor. In other words, the rate of discharge of impulses to voluntary movement though less than that of involuntary movement of tremor is yet related to it in such a way that to the more rapid rate of voluntary discharge there corresponds a more rapid rate of involuntary discharge. The correlation, however, is not universal: there are three patients out of the twelve who do not follow this rule. There is a further fact which becomes obvious on considering the individual data,—that mental ability is in no way correlated with speed of voluntary movement: three patients out of the four who have the highest rates are included in the group of more demented epileptics.

It will be remembered that the observations on tremor were made first with the arm supported and then with the arm free. The data given above refer only to the tracings taken when the arm was supported. The tracings taken when the arm was free, instead of being clearer, were in nearly all cases much less distinct and much more difficult to read. The numbers for the more and the less rapid groups in Table VII are 8·1 and 7·5 when the arm is free, but I do not wish to lay stress on these values as the lesser numbers may in some cases be due to the difficulty of reading the curves even when the tracing was magnified. The observations do not give grounds for deciding how far the lessened distinctness is due to the character of arm tremor, and how far it is due to interference and summation of arm and hand movements.

Rhythm and maximal contractions. The observations in regard to rhythmic movement follow the rule of the simple voluntary movement in showing no appreciable difference between the two groups of epileptic patients with whom alone the experiments were made. There are differences in the way in which the pressure on the rubber tubing is made, some giving a slight pressure, others striking the tubing with some force at each beat. But with the exception of two in the second group all the patients were able to follow both rates of the metronome with approximate regularity. A more complex rhythm might have brought out differences, but would probably have made too great a demand on the intelligence of the patients.

The differences in the way in which various subjects carry out a maximal voluntary contraction are marked. Neglecting the 'latent period' between stimulus and rise of the curve we find differences in the rise of the curve, in the apex or plateau, and to a less extent in the fall. And the form of the curve is a constant characteristic which persists through the series of 10 or 12 contractions recorded each day and reappears on successive days in most of the cases with little change. The following tracings of two successive curves from three of the patients will serve as illustrations of the way in which the effort is carried out. The lowest line gives the time when the signal for the effort was given: the line above that gives the time in fifths of a second: the curves are to be read from right to left.



So far as can be seen the records cannot with certainty be correlated with the features of the epileptic condition. It may however be mentioned that there is in the curves obtained from some epileptics after the fit an alteration which consists in a gradual and prolonged rise of the curve. Some patients seemed unable, however often the matter was explained, to let go when they had given their maximal contraction, and kept up their grip till told to let go; but the observations are not sufficiently numerous to allow of general statements. The various types of curve are found both in normal and epileptic subjects. Perhaps we have here, as in former tests, a comparatively simple form of action in which individual differences are the prominent feature and are relatively unaffected by the general epileptic condition. Further analysis of single voluntary contractions, which I hope to undertake, and correlation with other forms and conditions of action may bring out the significant features, and show how the curves are modified by the force and persistence of the central innervation, the correlation of antagonistic muscles and the peripheral sense of effort.

The fact that there is a certain amount of correlation among the results of certain of the tests which have been employed, while in others the correlation fails, is in line with the conclusions of other investigators. It will be sufficient to refer to the work of Wissler[1], dealing with the varied tests which have been applied to students of Columbia University. At the conclusion of his investigation he makes the statement that "in summing up the foregoing it appears that all the tests in this series have little interdependence." Wissler, however, does not regard this result as proving the uselessness of such tests, but urges that they should be carried still further, in the belief that "because of the problems raised and the suggestions offered for new hypotheses, the making of tests is more desirable than ever." This point of view appears to have ample justification, both from the scientific and the practical aspects. It is only by carrying on such investigations that we can hope to escape from the vagueness and uncertainty of our views regarding the factors that determine mental ability and the various types of character. Negative results have perhaps more value in such investigations than they have elsewhere, inasmuch as they force on our attention the richness and complexity of mental life.

Practice. Under this heading I wish to bring together the facts which have been noted regarding the way in which the results varied in successive experiments on one day and on different days. For this purpose the results of the two epileptic groups taken together have been analysed for the first three days in order to show what changes took place, and to afford a comparison with the normal results which cover only three days. In general it may be said that the effects of practice, as shown in the results of successive days, and of 'Anregung' as shown in the successive results of each day, though present, are of minor importance.

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[1] "The Correlation of Mental and Physical Tests," *Psychological Review*, Monograph Supplements, Vol. III. No. 6.

In the test for recognition there is a distinct and fairly regular improvement in regard to both factors, forgetting and confusion, as we pass from the first to the third day. The following table in which the two classes of errors are combined shows the daily changes.

TABLE VIII.

	First Day		Second Day		Third Day	
Subjects	Pictures	Words	Pictures	Words	Pictures	Words
Normal	2·8	2·4	2·2	3·2	1·2	2·4
Epileptic	4·4	5·6	3·7	4·4	2·0	4·1

As the improvement is clearly visible on the third day both in normal and abnormal subjects we have evidently something more than increasing familiarity with what they were expected to do, and may fairly consider the decrease of errors as being largely due to practice. It seems fairly certain that if the normal subjects had continued their work as long as the patients the differences between normal and pathological groups would have been still more marked than they are. In this test we are, of course, unable to see the effect of successive experiments on each day.

In the test for immediate memory, on the other hand, we find that there is practically no improvement as we pass from the first to the third day of the epileptic results, and that the average for the three days is very nearly the same as the final average. There is, however, an improvement as we pass from the first to the third of the experiments with epileptics on each day, the values of rightly-placed letters for the three observations being 19·7, 18·2, 17·3. Seeing that there is no improvement from day to day in the abnormal groups we can directly compare the results of the normal and abnormal groups.

In the results for choice the values of the patients for the three days are 29·9, 28·5, 26·7: there is, therefore, a distinct tendency towards shortening the reaction time, the tendency being rather more decided in the second epileptic group. There is a similar improvement in the normal group, the corresponding values being 18·5, 16·5, 16·6. There appears in the average values for the patients a very slight deterioration in the second experiment of each day: this, however, is due to the results of the first day; on the other days there is a small improvement in the second place. In the normal group the second experiment of each day takes on the average 1·1 sec. less time than the first.

In throwing cards there is as we pass from the first to the third day a gradual lengthening of the time in the case of the epileptics, which is due to the second group, while in the normal group there is a slight improvement. The second experiment on each day gives a slightly shorter time in both epileptic and normal groups.

In sensory discrimination we have the curious result that there is on the whole an increase of errors from day to day in the epileptic results, the figures being 10·4, 10·5, 11·6, and that the errors are more numerous in the second experiment of each day, the values being 5·3 and 5·5. One might be inclined to suspect these results as being accidental, were it not that one finds the same tendencies in the normal group in which the errors on the three days are 4·7, 5·5, 6·1, and in the first and second places each day are 5·3 and 5·6. It is rather difficult to give a reason for these peculiarities: it is, however, to be remembered that the results of this test have a character quite distinct from that of the other results, and we may perhaps correlate this anomalous feature in regard to practice with the other anomalous relations of the groups as shown by this test.

THE LIMITS OF GENETIC AND OF COMPARATIVE PSYCHOLOGY[1].

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SYNOPSIS.

The distinction between these two branches of Psychology.

Genetic Psychology, incompatible with the Humian position, presupposes as its 'unit' the conscious self. This involves distinguishing psychical development from biological. Individual and social factors of developing consciousness.

The initial problem of Comparative Psychology is to determine the criterion of consciousness. The 'continuity theory' maintains that life implies consciousness; the 'mechanistic theory' denies consciousness in organisms whose actions are unvaried reflexes. So far as they differ neither theory proves its own thesis or refutes that of its opponent: both agree in recognising consciousness where there are adapted reactions.

A survey of the relevant facts known shows that the scope of comparative psychology is after all as wide as animal life.

The problem of the nature of animal consciousness at different stages: in the minimal form of consciousness clearly ascertained, where truth and error generate the habit of purposeful reaction, imagination is implied. But even the higher animals seem only to react to concrete situations which they very imperfectly analyse. The question how far there is a social factor in animal development can only be settled when we have an objective test of true imitation.

Child psychology: the comparative method alone applicable to infancy: later the child's own introspection may supplement the study of its thoughts, imitations, and self-assertiveness.

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[1] This paper, substantially as here presented, was read before the Section of Comparative and of Genetic Psychology, at the International Congress of Arts and Science, in St Louis, on September 24, 1904.

As a mere student of general and adult psychology, and no first-hand investigator of the consciousness of animals, little children, and primitive men, I have no especial right to treat of this topic. I have been induced to do so by the conviction that comparative and genetic psychology, even more than other branches of the science, urgently need to-day a fresh survey of their boundaries, a definite formulation of their basal conceptions.

For such a critical consideration of fundamental problems, the general psychologist has, perhaps, a peculiar fitness. He may not penetrate, as the specialist can, into the thicket of the phenomena of animal behaviour, infant activity and primitive customs, but he may yet avoid the specialist's danger of failing to see the forest for the trees, of confusing general considerations with irrelevant details.

The terms 'genetic' and 'comparative psychology' are commonly employed without discrimination[1]. This, however, is an unjustifiable confusion. For genetic psychology is a mixture of comparative, or inferential, with direct, or introspective, psychology. Its study of the consciousness of animals and of primitive men, and much of its study of the child consciousness, are, it is true, pursued by the indirect or comparative method: the comparison of their behaviour and of their bodily structure with the conduct and the structure of the normal adult. But an introspective, yet genetic, study of successive phases of the conscious life of the adult may also be made, and such a study, though genetic, obviously need not be comparative. The distinctive feature of genetic psychology is, indeed, not its method—that may be direct or indirect—but a certain character of its subject-matter. The material of a genetic science is, in fact, anything in its development: whether it study life or consciousness, animal or child or adult, a genetic science studies this object as developing.

Logically antecedent to all special problems of genetic and of comparative psychology, is the question of their scope and conversely of the limits of each. These basal problems constitute the main subject of this paper. The first to be considered is:

I. *The Limits of Genetic Psychology.*

At the very start, genetic psychology is met by the challenge to its logical existence. It is seriously questioned whether there is any sense in which consciousness may be said to develop, in other words, whether there is room for any genetic science of consciousness. It is evident that consciousness, conceived after the Humian fashion as a succession of phenomena, or events,—a chain of ideas, in the widest sense of the word 'idea,'—cannot be said to develop. Development presupposes a unit of change, that is, some one reality which expands or narrows, grows or decays and yet retains, throughout its change, a certain identity. The Humian, 'linked-idea,' conception of consciousness has, as its only unit of reality, the single idea, that is, state of consciousness[1]; and this state of consciousness does not develop since it exists for the moment only. The Humian conception, because it recognizes no reality which underlies that of these evanescent states of consciousness, leaves no room to conceive of consciousness as developing. Consistent advocates of this system[2] rightly, therefore, reject the conception of a strictly genetic psychology. They concern themselves, it is true, with genetic biology, in the effort to explain concrete psychological phenomena. For example, they account for the fear of empty spaces by the fact that empty spaces were dangerous to our animal ancestors, and that therefore those of them who escaped, through instinctive flight, survived to propagate offspring. This, however, is no genetic psychology: it is a psychological use of the facts of genetic biology. Psychology, as study of succeeding states or processes of consciousness, is not, and may not become, a genetic science.

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[1] Cf., for example of this frequent misuse, D. G. Brinton, *The Basis of Social Relations*, p. 3, in which he describes comparative psychology in terms applicable to genetic psychology only. "Its province," he says, "is to trace the evolution of human mental powers to their earlier phases in the inferior animals."

A genetic psychology is possible, in truth, only on a radically different view of the nature of consciousness. According to this second doctrine, consciousness is not a mere succession of ideas; it is a self, conscious of itself, of other selves and of its ideas. The unit of psychology is, from this standpoint, not the single state of consciousness, or idea, but the conscious self. This is not the time in which to expand this conception and to distinguish it sharply from the metaphysical doctrine of the self, with which it is too often confused. It is essential, however, to suggest the justification for such a conception of consciousness. The ground of the doctrine is, briefly, this: Introspection discloses that every state of consciousness is immediately known as the state, or idea, of some self. Thus, the consciousness of any idea involves also the consciousness of a self, conscious of this idea. On immediate introspection, therefore, and not on philosophical demonstration, this conception of consciousness is based.

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[1] *Treatise on Human Nature*, Bk. I. Pt. IV. 6.

[2] Cf. Münsterberg, *Grundzüge*, Kap. XIII.

It seems evident that a self, thus conceived, does undergo a development, which is psychical and not physiological. The progress from infancy to maturity, or the advance from the savage through the barbaric to the civilized epoch of human life, illustrates what is meant by self-development—a change which is correlated with physiological processes but is surely not identical with them[1]. My ten year old self, shrinking with a terror which I still revive, from a new face, and my present self which welcomes every stranger with a genial interest, are related by an identity and a difference. I am the same self—the childish terror still is realized as mine—and yet I am another self with a new attitude toward the world. In just this progressive complexity of sameness and difference, self-development consists.

To psychical development, thus conceived, as characteristic of a self in its relations, there is not, of course, the decisive difficulty which faces the theory of psychical development, when consciousness is looked upon as series of ideas. An idea, a momentary psychical occurrence, lacks—as has been pointed out—the permanence which is necessary to a developing reality. A self, on the other hand, has both identity and permanence, and may, therefore, be conceived as growing and expanding. Yet the theory of self-development has its own characteristic obstacle: a self, it is urged, is not a temporal reality at all; on the other hand, time is the characteristic category of the idea, and a self must be regarded without reference to temporal realities. Only in its connexion with its body, is it necessarily considered as a past or present or future self: essentially it is the same self, in spite of these distinctions[2]. And, if this be true, the development conception may not be applied directly to a self.

It must be admitted that this difficulty has its roots in a just conception. Selves are, in truth, primarily untemporal; and their basal relations are the untemporal ones of common experience, of sympathetic emotion, of self-assertion or of loyalty to others. It is therefore impossible to dispose of this objection to the theory of self-development on the ground that it is a purely metaphysical consideration. For psychological analysis, as well as metaphysical reflection discloses this untemporal quality of conscious selves.

On the other hand, this difficulty cannot be admitted as a final one.

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[1] Cf. E. C. Sanford, "Mental Growth and Decay," *American Journal of Psychology*, Vol. xiii. pp. 426 *seq.*, 1902.

[2] Cf. Münsterberg, *Grundzüge*, Kap. iii. (1) und (2); and Royce, *World and Individual*, Gifford Lectures, Second Series, iii. and vi.

A scientific study of selves, in untemporal relation to each other, is indeed possible; but the temporal relation of selves to their own past and future, no less than to the temporal experience of other selves, must be admitted as a possibility. For science is, after all, but a systematisation of the every-day consciousness; and the facts of self-development are unquestionably matters of every-day observation. In other words, a self may be regarded, from the scientific, as perhaps contrasted with the metaphysical point of view, now as temporal and again as untemporal; now as presenting a complexity of untemporal qualities and relations, again as presenting a succession of unfolding phases.

Genetic psychology, the study of developing selves, is then a legitimate science. This admission leads the way to a more precise definition of development. At the outset, it must be insisted that development, in the technical sense of evolutionary biology, cannot possibly be predicated of conscious selves. This will be admitted when it is considered that heredity and natural selection are essential features of biological development. But it is simply impossible that a self should transmit its characters to another self. Every self, on the other hand, has a certain independence and self-identity, which is quite incompatible with a transmission of characters. The traditional conception of psychical heredity, as set forth, for example, by Ribot[1], is nothing more nor less than an empirical observation of the psychical likenesses between children and their parents[2]. In this sense of similarity, psychical heredity is, of course, a fact. Such similarity does not, however, involve a transmission of mental characters. The conception of development, so far as it includes heredity, in this stricter sense, is applicable, not to conscious selves, but to animal bodies.

This ruling out of the factor of heredity sufficiently proves it impossible to conceive the self as developing in biological fashion. This position is strengthened by the discovery that natural selection, the preservation of certain individuals through the destruction of others, is altogether incompatible with one important tendency of psychical progress. This is the social tendency toward protection and help of the weak. Every discussion of human evolution emphasizes this 'failure of natural selection[3].' The weak, who could not, unaided, win in the struggle for existence, are cherished and protected and preserved, by parental care, philanthropic effort or religious zeal, in opposition to the workings of natural selection.

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[1] *L'Heredité Psychologique*, 1902.

[2] Cf. Ribot's virtual admission of this, *op. cit.* p. 387.

[3] F. T. Headley, *Problems of Evolution*, pp. 282 *seq.* and 384; L. T. Hobhouse, *Mind in Evolution*, ch. XIV. pp. 387 *seq.* and 395.

Development in its well-marked biological sense is, then, impossible within the domain of related selves. It is important to reiterate this conclusion in the face of the cheap and easy theories of transmitted traits and of natural selection working in the sphere of social relations. But development is not necessarily conceived after the fashion of biology[1]. In a broad sense, development may be defined as the succession of more complex upon simpler states, or, conversely, of simpler upon more complex states of a unitary being. Such a psychical development, the normal adult who remembers his own past states may introspectively observe in himself[2].

Genetic psychology, thus conceived is, evidently, in the first instance individual psychology: it is the study of the individual self—human or animal, child or adult—in the process of learning, in the progress from simple to complicated consciousness, or again in the reverse order from complex to simple. This learning consciousness, as will later appear, is of two chief types: individual or social. That is to say, a developing self either profits mainly by its own experience, without realized relation to other selves, or it learns by its attitude, imitative or inventive, toward these others. Each of the forms of learning, individual and social, has—in turn—two subdivisions. The individual learning consciousness is either mainly an associative or it is an analytic, reasoning process. And the social type of learning, by imitation and opposition, is distinguished, as the imitation is of contemporary or of past selves. In the latter case, genetic psychology may, indeed, be called a racial psychology. The child imitates his parents and the parents have learned through imitation of an earlier generation, and each imitation has involved an allied origination, a supplementation of the copy. Material for a genetic race-psychology is found, thus, in the indirect dependence of an individual, through imitative and inventive consciousness, on individuals who, from the temporal standpoint, have preceded.

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[1] Cf. Hobhouse, *op. cit.* pp. 375 *seq.* and 388 *seq.*

[2] This conception of psychical development should be distinguished from a doctrine, recently formulated by J. M. Baldwin, who maintains that the science of development, rightly regarded, is psychophysical, that is, that it has to do neither with the psychical nor with the physical exclusively, but with the whole psychophysical organism—the composite of soul and body. From this point of view, Baldwin says, "the question of genesis" is a question of "the development and evolution of mind and body taken together. Changes in mind and body," he continues, "go on together and together they constitute the phenomena" (*Development and Evolution*, p. 8; cf. pp. 10, 129).

On this theory, the following comments may be made. Evidently, in the first place, it is not, as Baldwin claims, a form of parallelism, but rather a variation of interactionism. In the second place, the conception on which the theory rests, that of a psychophysical organism, possesses a questionable validity. It may be doubted whether such a conglomerate of psychical and physical does not actually affront the every-day experience which it has claimed as its chief warrant; whether, in other words, the attempt to combine the two must not end in the reduction of one to the other. Finally, even granting the validity of the conception, such a study of genesis surely is biological, not psychological.

These distinctions will be illustrated in the second section of this paper. The preceding pages have attempted to vindicate the existence of genetic psychology, and to outline briefly both its scope and its limits. It will be necessary to discuss at greater length the problem of:

II. *The Limits of Comparative Psychology.*

The bare existence of comparative psychology is nowadays called in question by no one. Practically all psychologists admit the consciousness of animals, of infants and of primitive men, and consequently grant the right to existence of a science which shall study these forms of consciousness. The limits of comparative psychology are, simply, then, limits in the application of the comparative method. Its initial problem is: what classes of animals, or what stages of child life, or what levels of primitive life may be regarded as conscious? To the consideration of this problem, the remainder of this paper is chiefly devoted. For lack of time, the problems of ethnic psychology will be utterly neglected, and the main stress will fall on the facts of animal psychology. The analysis of the consciousness of animals and of infants will be incidentally undertaken.

To determine what sorts of animals and what stages of the child-life are conscious, it is necessary to formulate some objective or indirect test of the presence of consciousness. Even if, as in common with many psychologists, the writer believes, the direct consciousness of other selves is involved in every self's consciousness of itself, such a direct consciousness evidently is not present in the mere scientific study of animals, of children and of primitive peoples. That these organisms are conscious is not directly or positively known: it is affirmed or denied by inference from structure, behaviour or product. A basis for this inference, that is, a test of the presence of consciousness, has therefore to be found.

There are two widely opposed doctrines concerning the nature of this test. The first of these—the continuity doctrine, as it may be named—virtually holds that the evidence of consciousness is the possession of an organized body; it teaches that all animals are conscious, on the ground that life implies consciousness. In other words, the continuity doctrine interprets animal movements by their analogy with human conduct. Opposed to this conception, is the mechanistic theory which conceives of all plants and of the lower forms of animal life as mere mechanisms, on the ground of their unvaried and recurring reactions. The test of consciousness, according to this theory, is non-mechanical—that is, adaptive—conduct[1].

There are two arguments for the conclusion that consciousness is a property of life. The first of these is a metaphysical argument based on that principle of continuity from which I have named the doctrine. Some animal organisms, it is pointed out, are admitted to have consciousness; and the forms of animal life, it is urged, succeed so closely on each other, that it is irrational to suppose the sudden appearance of consciousness at any one stage; hence it must have accompanied animal life from the first.

The second argument is empirical, not metaphysical, in character. It is based on a two-fold analogy of the animal with the human organism: the likeness of certain structures of animal bodies with those of human bodies, and—more important—the similarity of animal activities to movements consciously performed by human beings. This analogy is extended to the lowest forms of animal life. "The Infusory," Binet says[2], "guides itself while swimming about; it avoids obstacles; often it undertakes to force them aside; its movements seem to be designed to effect an end.... In short the act of locomotion as seen in detached Infusoria, exhibits all the marks of voluntary movement." After the same fashion are explained the movement of bacteria toward grains of chlorophyll, in a drop of water which contains no oxygen[3], the amoeba's rejection of foreign substances[4], and the movements of animals higher in the scale of life—of echinoderms, for example—from dark places into well-lighted ones[5].

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[1] There is also a modified form of the continuity theory, well represented by G. H. Schneider. It argues the psychic nature of most animal movements, from their purposiveness, but admits the purely physiological and non-psychic nature (1) of plant processes, and (2) of certain movements of the animal body—that of the iris, for example. The objections to this modification of the continuity doctrine are, first, the arbitrariness of assigning consciousness to low forms of animal life, while denying it to practically indistinguishable vegetable organisms; and, second, the improbability of sharply distinguishing purposive from purposeless animal movements.

[2] *The Psychic Life of Micro-organisms*, Translation (Open Court Co.), p. 46.

[3] *Ibid.* p. 32.

[4] *Ibid.* p. 41.

[5] G. H. Schneider, *Der thierische Wille*, Kap. v. S. 164.

In all these cases, the underlying argument is the same: infusoria, bacteria and amoebae, echinoderms and higher animals perform acts like those which are voluntarily, or at any rate consciously, carried out by human beings; therefore infusoria and the rest are conscious. The analogy is strengthened when to similarity of conduct is added likeness of structure. So, Binet describes[1] the ocular spot of a ciliated infusorian as "composed of a convex crystalline humour, having the form of a watch crystal enveloped by pigment," and concludes that "the identity of structure" with that of the eyes of higher animals "naturally leads to the assumption of identity of functions."

An examination of these arguments discloses, however, certain flaws, of which the more significant are the following: In the first place, the law of continuity proves too much. If the sudden appearance of consciousness at any stage of organic evolution be irrational, so also is the sudden appearance of consciousness at any point of development—inorganic as well as organic. Consciousness must, in other words, if ascribed to vegetable as well as to animal organisms, be claimed, on the same principle, for inorganic matter: it must be attributed no less to the iron-filings attracted by the magnet than to the infusoria attracted by the food.

The chief objection to the empirical argument of the continuity theory is less easily stated in a compact form. This argument, it will be remembered, rests on the analogy of such acts as the movements of bacteria towards the light, or the hiding of young gulls in crevices of the rocks, with corresponding acts of human beings, which—it is assumed—are consciously performed. But this assumption is obviously in fault, so far as individual human acts are concerned. Motions even more complicated—for instance, the avoidance of obstacles in walking and the muscular contractions necessary to a musical performance—may be unconsciously performed by human beings; and their likeness with human movements cannot, then, prove animal actions to be conscious. To rescue the analogy, the modern upholders of the continuity theory, therefore, make a further hypothesis. They admit that even complex human acts may be unconsciously performed, but they conceive all such acts as habitual, and they assume that all habitual acts were originally performed with consciousness, and have grown unconscious with practice[2]. Many of these unconscious human movements may, it is shown, be traced back to acts consciously performed in the past life of the individual. Others, for example, the characteristic bodily 'expressions of emotion,' have remained unconscious throughout individual experience; but these, it is urged, were consciously performed by the animal ancestors of the human being: in a word, they have grown unconscious through racial experience.

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[1] *Op. cit.* p. 28.

[2] Cf. Spencer, *Principles of Psychology*, Part IX. chap. IV. Vol. II. pp. 546 *seq.*; Schneider, *op. cit.* Kap. V. S. 153; Wundt, *Grundriss*, IV. § 19, 4, S. 389; E. B. Titchener, *Popular Science Monthly*, Vol. LX. pp. 465—467, 1902.

The complete argument, restated in accordance with this fresh hypothesis, is then the following: All animal activities are psychic because they resemble human activities, and all human activities are psychic because they either are, or originally were, performed with consciousness.

But, even in its new form, the argument is invalid. In the first place, admitting its premises, it does not prove of any particular animal activities that they are conscious, but only that they once were conscious—though perhaps not so in the experience of the individual. In the second place, the conception of unconsciousness as attained through racial practice, involves the assumption that the effects of practice are transmitted, and has, thus, to reckon with all the formidable objections brought to bear against this revival of Lamarckianism. Finally, even if this difficulty be waived, the continuity argument loses all its force with the appeal to racial experience. For the strength of the argument lies in the analogy between animal behaviour and directly observed human consciousness. When once, however, it is admitted that some human activities are, and have remained, unconscious throughout individual human experience, then the analogy becomes a futile one. To call the animal reactions psychical because analogous to human actions, when these human actions are psychical only on the hypothesis that they once were consciously performed by the animal ancestors of human beings—this is an obviously circular argument.

The admission of this radical defect in the argument of the continuity theory leads naturally to a consideration of the teaching of the mechanists. This doctrine, it must be remembered, is less sweeping than the one which it opposes; for that asserts that all forms of animal life are conscious. The mechanists are far from teaching that all animals are unconscious. They insist, however, that those animals only are conscious which behave in a non-mechanical way, those whose reactions vary in such wise as progressively to adapt themselves to their own environment. Negatively, then, they contend that those animals are unconscious whose movements are repeated and unvaried reflexes; and after this mechanical fashion, they interpret many of the activities, which their opponents regard as psychical, notably the merely instinctive activities of animals[1]. Their argument, also, is two-fold, theoretical and empirical. As against the metaphysical law of continuity, they urge the logical law of parsimony—the alleged necessity of explaining every phenomenon in the simplest possible way. Now the physiological antecedent of a movement has always to be admitted, so that the introduction of consciousness, as part-condition of a movement, certainly adds a complicating factor. It follows, on this principle of parsimony, that any movement which can be explained as unconscious should be so explained; in other words, that the simplicity of the unconscious-reflex conception gives it right of way.

The empirical argument of the mechanists is, in the main, the development of a counter analogy, namely, the likeness of the supposedly purposive and conscious acts of animals with plant changes and with mechanical processes. The argument has three distinguishable forms. In the first place, it has been experimentally demonstrated, by Loeb[2], Bethe and others, that many animal activities which have been called psychical are, in reality, direct responses to mechanical, chemical or electrical stimuli. Even the homeseeking activity of ants, Bethe has shown[3], depends on the traces of food which have been dropped on the outward road. The homeseeking is, thus, Bethe argues, an unconscious reaction to a chemical stimulus.

It has been pointed out, in the second place, that certain instinctive animal activities, usually conceived as conscious, on account of their purposive character, are persisted in when they become useless and even positively harmful. Loeb, for example, has experimented on the crevice-crawling instinct of animals, which is ordinarily cited as an activity performed for the purpose of self-preservation, a movement to escape pursuit by seeking obscurity. Loeb, however, finds by ingenious experiments on butterflies and on worms, that the instinct persists when the crevice is brightly lighted and therefore at least unfitted for a place of concealment[1]. By similar experiments, Yerkes has shown that certain crustaceans persist in a hurtful instinctive activity[2]; and Jennings has proved the same for several forms of infusoria[3].

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[1] This mechanistic theory must not be confounded with the theory that consciousness is identical with nerve-process, a doctrine current among certain modern physiologists —one may instance Loeb (cf. *Comparative Physiology of the Brain and Comparative Psychology*, pp. v, 12, 218). This revival of the outworn doctrine of Vogt and Büchner is obviously a result of defective observation and of the unwarranted intrusion of a one-sided metaphysic into psychology.

[2] Loeb lays great stress on the position that the nervous system, so far from being functionally distinct from mere protoplasm, only "plays the part of a more sensitive and quicker conductor for the stimulus." This, though well established (cf. Loeb, *op. cit.* pp. 4 *seq.*, 38 *et al.*; Goltz u. Ewald, *Pflüger's Archiv*, Bd. LXIII. 1896, S. 874; Schrader, *Pflüger's Archiv*, XLI. 1887, S. 75 *seq.* and Bd. XLIV. 1888, S. 175 *seq.*), has no direct bearing on the present discussion, except for those who start out from the *à priori* conviction, that nerve excitation is an inevitable and theoretically necessary condition of consciousness.

[3] "Dürfen wir den Ameisen und den Bienen psychische Qualitäten zuschreiben?" *Pflüger's Archiv*, 1898, Bd. LXX. S. 15 *seq.*

In the third place, the direct analogy is emphasized, between animal reflexes and instincts, on the one hand, and inorganic as well as vegetable processes, on the other. The upward crawling tendency of young caterpillars and the upward migrations of pelagic animals are mere mechanical responses, Loeb insists, "similar to that which forces the stem of the plant at the window to bend toward the source of light[4]," and the changes produced by electric currents in the postures of amblystoma are closely analogous to the phenomena observed in inorganic bodies, under the influence of electric currents[5].

A careful examination of these three arguments will disclose, however, that no one of them is an entirely decisive proof that instinctive animal activities are unconscious. To begin with the argument last stated: the likeness of instinctive animal reactions to inorganic processes stamps these animal reactions as non-psychical, only on the supposition that inorganic movements are non-psychical. This assumption is, to be sure, commonly made; and it is made, in particular, by most of those who support the continuity hypothesis. By them, therefore, the logic of this argument from the analogy of animal with inorganic movements may not be challenged. Any one who argues, "The echinoderm, like the human being, turns toward the light and is therefore conscious," cannot object, on formal grounds, to the counter argument, "The nereid crawls into the lighted tube as inevitably as the needle turns to the pole, and is therefore unconscious." From metaphysical considerations, on the other hand, this argument might, none the less, be questioned. In the second place, actions which are mechanically repeated, even when useless or dangerous, are surely not purposive. Nevertheless this argument also falls short of its ulterior aim. Acts which are purposeless may still quite well be conscious; and the caterpillars, worms and butterflies may have the sense-consciousness of crawling, even without the purpose of self-preservation. So, again, the successful proof that animal reactions are direct responses to an external stimulus disproves the selectiveness, but does not disprove the consciousness of their acts. Bethe's ants, for example, though incapable of finding their way home, by means of visual imagination, may none the less have been conscious of odour, as they followed the trail of the food dropped by the way. And, finally, the law of parsimony, the theoretical stronghold of the mechanist, cannot be admitted as a controlling consideration. It is true that, all other things being equal, the simplest explanation is to be preferred, but no methodological consideration can be allowed to determine a question of fact; nor—in the opinion of the writer—can it have the authority of a metaphysical principle. In other words, granting the equal significance of each in its own sphere, the mechanist's methodological law of simplicity ought not to outweigh the metaphysical law of continuity.

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[1] *Op. cit.* pp. 184—185.

[2] R. M. Yerkes, "Reaction of Entomostraca to Stimulation by Light. II. Reactions of Daphnia and Cypris," *Amer. Jour. of Physiology*, 1900, Vol. IV. p. 419.

[3] H. S. Jennings, "Studies on Reactions to Stimuli in Unicellular Organisms. II.," *Amer. Jour. of Physiology*, 1899, Vol. II. p. 330: cf. citations on p. 275, below.

[4] *Op. cit.* pp. 181 *seq.* Cf. "Der Heliotropismus der Thiere u. seine Uebereinstimmung mit dem Heliotropismus der Pflanzen," 1890, S. 93 *et al.*

[5] *Op. cit.* p. 166. Cf. Loeb and Garrey, "Zur Theorie des Galvanotropismus. II.," *Pflüger's Archiv*, Bd. LXV. 1896, pp. 41 *seq.*

Evidently, then, the mechanist does not make his point. He has tried to show that unvaried reflex movements are unconscious, but has succeeded only in proving that they may be unconsciously performed, and that it is simplest to conceive them thus. His opponent, however, is not more successful. He has tried to prove that all animal activities are *ipso facto* conscious; but has shown merely that they may be conscious and that there is a certain metaphysical support for this conclusion. With regard to organisms whose reactions are unvaried reflexes, it thus appears that neither doctrine has been justified, on purely scientific grounds. The one proves the possibility that these fixedly reacting animals may be conscious, but does not empirically disprove the possibility of their unconsciousness; and the other empirically proves the possibility that they are unconscious, but does not disprove the possibility of their consciousness.

It is, however, highly important to observe that this unsettled issue does not concern the animals which make adaptive movements. For even the mechanists are agreed that non-mechanical behaviour, that is, adaptive reaction to a fixed environment, is a sufficient guarantee of the presence of consciousness. Verworn[1], Bethe[2], and Loeb[3] unite in admitting this test of consciousness. "Es scheint mir," Bethe says, "der Nachweis ob ein Wesen im Stande ist modificiert zu handeln der einzige Prüfstein zu sein, um auf psychische Qualitäten zu schliessen." It is, therefore, wise to waive the disputed question, does consciousness belong to animals whose reactions are mere fixed reflexes; and to ask, instead, what animals meet that test of consciousness which is admitted by all—in other words, what animals make adaptive reactions. It is scarcely necessary to make the preliminary observation that this argument from the occurrence of adaptive movements constitutes no absolute proof of the presence of consciousness, since the argument rests on a mere analogy. Yet the completeness of this analogy of the adaptive animal acts with conscious human movements, and the utter absence of an analogy with mechanical processes combine to justify the conclusion that animals which react adaptively are conscious. Fortified by the metaphysical argument from continuity, and by the conviction gained by direct relations with the higher animals, we do admit, with practical unanimity, that animals who react in varying fashion to a fixed environment are conscious.

It is well, at the outset, to illustrate with more care the exact nature of this criterion of consciousness. It has been described already as the occurrence of adaptive or varying reactions. Not only are the movements of animals at this stage discriminative—that is, are different for different environments—but they are also varied: in other words, though the environment remains unchanged, the animal, far from responding with one fixed movement, reacts with progressively increasing complexity and effectiveness[4]. The dogs and cats and rats, for example, in the experiments of Thorndike and Small and Watson, who succeeded in learning to let themselves either out of boxes or into them, by unfastening latches of varying difficulty—all these at first reacted to the stimulus of the closed door by a relatively fixed, indiscriminate response—for instance, by clawing at the whole surface of the door. Among these repeated and ineffective movements, however, a specialized movement presently occurred, which drew the latch; and this successful movement was repeated, at first by accident; and the casual repetitions of the movement resulted at length in the formation of a new motor response, a variation of the original response to surrounding conditions. Adaptive variation, thus described, is—it will be observed—identical with the first stage in the developing consciousness of a self—the process of learning through trial and chance success. This criterion of the presence of consciousness implies, in other words, the development of consciousness.

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[1] *Protisten-studien*, S. 137 u. S. 141.

[2] *Op. cit.*, *Pflüger's Archiv*, Bd. LXX. S. 19.

[3] *Comparative Physiology, etc.*, pp. 12 *et al.*

[4] Cf. below, p. 278.

In passing, this criterion of consciousness should be distinguished from two others, current in psychology. By some psychologists, as for example by Schneider, a purposive—not a purposed, but a useful—act is regarded as psychical in its nature. There are two objections to this doctrine. In the first place, it defines the non-mechanical from the standpoint, not of the reacting organism but of the observer; in the second place, it makes the unwarranted implication that there are simple reflex actions which are not purposive. A second criterion of non-mechanical, that is, conscious, movements has recently been proposed by Dr Minot[1]: the ability to 'dislocate' reactions, in time—that is, to delay reactions to a given stimulus. To this, it has reasonably been objected that "unconscious mechanisms could be constructed, and indeed do exist in which there is a dislocation in time between the action of an outer agent—and the reaction of the machine[2]." This time dislocation is, in fact, merely a subordinate form of that general ability to vary the response to a fixed environment, which constitutes a non-mechanical, and thus an admittedly conscious act.

We are now ready for the definite question: what animals have been proved to react adaptively, that is, to learn? To begin with the unicellular animals: Jennings has shown that most of them make a fixed and utterly unvaried response to a given environment. Many of them even react in but one way to all surroundings, giving the same response to every stimulus, however localized. The paramecium, for example, as described by Jennings[3], "responds to any stimulus by swimming backward, turning to one side and then swimming forward.... The direction of motion is the same whether the source of stimulation is at the anterior end, the posterior end, the side.... If the stimulus is at the posterior end [the animal] swims toward it, even though this results in [its] destruction." Other organisms of this class have more than one form of reaction, but react invariably by the same reaction to a given stimulus: the changed response is, in other words, called out only by altered surroundings. In the Dileptus, for example, and the Loxodes—forms of Ciliata—the reaction varies slightly with differently located mechanical stimuli[1]. A mechanical stimulus in front, causes the Dileptus to move backward, whereas a posterior stimulus is followed by forward motion. The essential feature of both types of reaction—of the indiscriminate and the discriminate response alike—is the fact that in both the movements are fixed and unvarying, if the environment do not change. There is, however, at least, one experimentally proved exception to the rule that unicellular animals react in this unvaried fashion. This is the case of fixed infusoria, Stentor and Vorticella, which not only vary their response to different stimuli, according as these are harmful or beneficial, but which alter markedly their response to a given stimulus, when that is harmful and prolonged[2].

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[1] "The Problem of Consciousness in its Biological Aspects," *Popular Science Monthly*, 1902, Vol. LXI. pp. 289 *seq.*, esp. p. 293.

[2] H. S. Jennings, "Studies on Reactions to Stimuli in Unicellular Animals. IX.," *Amer. Jour. of Physiology*, 1902, Vol. VIII. p. 57.

[3] "Studies on Reactions to Stimuli in Unicellular Organisms. II.," *The Amer. Naturalist*, Vol. XXXIII. p. 386, 1899.

Observation and experiment have, however, had to do mainly, not with unicellular organisms, but with animals of more complex structure. Romanes and Loeb have studied the radiates. Romanes attributes sense consciousness both to jelly-fish and to starfish, but he reaches this conclusion from the disputed standpoint of the continuity hypothesis. "The starfish," he says, "perceived the proximity of food, as shown by their immediately crawling towards it[3]." He thus argues consciousness from simple reaction, instead of experimenting on the starfish to discover if they would not react similarly to unodorous stimuli. Other experiments on different orders of Radiata have not, to my knowledge, resulted in the discovery that they make adaptive reactions, though Loeb's experiments offer evidence of their discriminate movements. The tentacles of actinians, for example, draw in meat or chemically treated paper, and reject ordinary paper; but, so far as is reported, they do not handle the meat or paper with increasing expertness[4].

Studies of molluscs, also, have—so far as they are known to the writer—consisted in examination of their sense-organs, in the study of the function of their nerve-systems and in experimental observations of their direct reactions; but have not included any efforts to train them to adaptive reactions[1]. None the less, Loeb concludes that cephalopods may possess associative memory[2]. It is greatly to be wished that some investigator might study different orders of the radiates and the molluscs, as Jennings has studied unicellular organisms, with the express aim of discovering whether they may be trained to adaptive reactions. For herein, and not in the possession of acute sense organs or of discriminated yet invariable reactions, is the trustworthy test of consciousness.

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[1] H. S. Jennings, "On the Movements and Motor Reflexes of the *Flagellata* and *Ciliata*," *Amer. Jour. of Physiol.* 1900, Vol. III. pp. 242 *seq.*

[2] Jennings, "Studies on Reactions to Stimuli in Unicellular Animals," *Amer. Jour. of Physiol.* VIII. 1902, pp. 23 *seq.*, esp. 52 *seq.*

[3] *Jelly-fish, Star-fish and Sea-urchins*, p. 321.

[4] Loeb, *op. cit.* chap. IV.

Different orders of the articulates have been studied with this precise end in view, and have been trained to simple motor habits. Loeb[3], to be sure, interprets the movements of worms as mere reflexes, but he argues mainly from the inconclusive fact, that their most complex reactions seem not to require coordination in brain centres. Yerkes[4], Yerkes and Huggins[5], and Spaulding[6] have experimented on Crustacea, and have proved that both crayfish and crabs can learn, by the trial and error method, to find their way through simple labyrinths. Spaulding's experiments are noteworthy since he trained his crabs to a habit opposed to their instinctive heliotropism, namely to seek food in the dark[7].

Close observation and careful experiment have been devoted to the study of insect behaviour. Bethe alone argues energetically that the movements of ants and bees alike are unvarying responses to environment—usually to chemical stimulus. But the position clearly is untenable. It is contradicted by such observations as that of Forel and others, "that wasps, etc., can find their way in flight through the air, notwithstanding mud and rain (and hence under circumstances precluding the existence of any possible odoriferous trail[8])."

[1] J. W. Spengel, "Die Geruchsorgane u. das Nervensystem der Mollusken," *Zeitschrift f. wissenschaftliche Zoologie*, Bd. xxxv. S. 333; W. A. Nagel, *Bibliotheca Zoologica*, Heft 18, 1894: "Ueber den Geruchs- und Geschmacksinn." Cf. SS. 163 *seq.*: "Landschnecken."

[2] *Op. cit.* p. 227.

[3] *Comparative Physiology, etc.* ch. VI.

[4] *Biological Bulletin*, III. 5 Oct. 1902.

[5] *Harvard Psychological Studies*, 1903, I. pp. 565 *seq.*

[6] *Journal of Comparative Neurology and Psychology*, Vol. XIV. pp. 49 *seq.*, esp. p. 53.

[7] The opposed results of Bethe's experiments on crabs, quoted by Yerkes in the *Harvard Psychological Studies*, I. p. 565, from the *Archiv für mikroskop. Anatomie*, are evidently due mainly to the insufficient number of the tests (five or six).

[8] *Ants and some other Insects*, p. 18 (Open Court Publishing Co.). Cf. Forel's ingenious experimental proof of the memory (not, as he thinks, of the inference) of bees, *op. cit.* pp. 22—27. Loeb also offers (*Comparative Physiology, etc.* pp. 225 *seq.*) a detailed account of a wasp which returned to its nest under conditions which shut out the hypothesis of reflex action.

Advancing to the vertebrates we find a number of experimental observations. Triplitt has experimented on perch, repeating and supplementing the classic experiment of Möbius on pike. He finds that perch may learn both to avoid one side of an aquarium and to inhibit the instinct to feed on smaller fish[1]. Yerkes has, so far as I know, performed the only experiment on reptiles. His turtles learned to find their way through simple labyrinths, by perpetuating successful movements—even the chance movement, unforeseen by the experimenter, of falling from the side of an inclined plane, instead of crawling to the base[2].

To the well-known experiments of Spalding, Morgan and Thorndike on young chickens, Porter has added a set of experiments on the English sparrow[3]. The investigators all agree that birds learn through progressively varying reactions. Thorndike's chicks learned to open the fastenings of boxes; Morgan's chicks and pheasants were taught by experience to avoid distasteful food; and Porter's sparrows learned to find their way through labyrinths.

For the mammalia, Small[4], and Watson[5], and Miss Allen[6] have proved that rodents learn to make their way through labyrinths of varying difficulty. Morgan[7] and Thorndike[8], Mills[9] and Hobhouse[10], though they differ in interpretation, agree as the result of independent observation and experiment that dogs and cats learn, by the perpetuation of accidentally successful movements, to open difficult fastenings or to find their way through strange paths. And, finally, Thorndike[11], Kinnaman[12] and Hobhouse[13] have experimented on different species of monkeys, and have found that they learn more swiftly and with greater accuracy than the other vertebrates, to perform relatively complex acts.

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[1] *American Journal of Psychology*, 1901, Vol. xii. pp. 354 *seq.*

[2] *Popular Science Monthly*, 1901, Vol. lviii. pp. 519 *seq.*, esp. p. 524.

[3] *American Journal of Psychology*, Vol. xv. pp. 313 *seq.*, 1904.

[4] "The Psychic Development of the Young White Rat," *Amer. Jour. of Psychology*, 1899, Vol. xi. pp. 80 *seq.*; and "Experimental Study of the Mental Processes of the Rat," *ibid.* Vol. xi. pp. 133 *seq.*, Vol. xii. pp. 206 *seq.*

[5] *Animal Education.* "The Psychical Development of the White Rat, correlated with the Growth of its Nervous System." University of Chicago Press, 1903.

[6] "The Associative Processes of the Guinea-pig," *Journal of Comparative Neurology and Psychology*, 1904, Vol. xiv. pp. 293 *seq.*

[7] *An Introduction to Comparative Psychology*, chaps. 5, 7, 12, 14, 16.

[8] "Animal Intelligence," *Psychological Review* (Monograph Supplement, viii.).

[9] "Animal Intelligence," Part iii.

[10] *Mind in Evolution*, chs. vii—ix.

[11] "The Mental Life of the Monkeys," *Psychol. Review* (Monograph Supplement, xv.).

[12] "The Mental Life of two *Macacus Rhesus* Monkeys in Captivity," *Amer. Jour. of Psychology*, 1902, Vol. xiii.

[13] *Op. cit.*

It follows from these facts, so far as they may be considered to be firmly established, that the scope of investigation in animal psychology is as wide as that of animal life. No form of animal organisms is *à priori* excluded, by its structural simplicity, from the group of adaptively reacting animals. Distinctions of behaviour and of consciousness, in other words, are not closely parallel with those of structure. Every order of animal life may therefore be studied with the possibility in view of discovering indications of non-mechanical, adaptive behaviour and, by consequence, of psychical life.

The boundaries of comparative psychology are, from this point of view, very wide. If, however, one consider, not the number of animals that are conscious, but the nature of their consciousness, the conception—viewed, thus, in intension, not in extension—shrinks to lesser limits. This problem of the nature of the consciousness of the different orders of animals can be here discussed in briefest outline only.

The minimal consciousness which an animal can be proved to have, is, as has been shown, the consciousness which accompanies the trial and error type of learning. This process should, therefore, be carefully scrutinized. On its physiological side—so far, that is, as observed behaviour and inferred nerve process are concerned—this sort of learning includes the following stages:—(1) There occur purely instinctive reactions to the environment, for example, the rapid running, hither and yon, of a rat, at the entrance of a labyrinth with food at the centre of it. (2) Among these varied instinctive reactions, there chances to occur a successful one, a movement, in other words, which secures the satisfaction of some instinct—in this case, the attainment of food. (3) This movement which satisfies the instinct is repeated, presumably more than once. The reason for the repetition need not here be discussed. The interactionist would of course explain it as a result of the pleasure which accompanies the satisfaction of instinct; the opponent of interaction, while admitting the occurrence of the pleasure, would explain the repetition as an effect of some physiological function of the satisfied instinct, an effect, perhaps, of heightened nerve vigour. Whatever the condition of the repetition, its result must be (4) a strengthening of the nerve connexion between the nerve centre stimulated by the original environment and the nerve centre whose excitation has brought about the successful movement.

In other words, the animal forms the habit of reacting to the original environment, by that movement only which satisfies desire. The present question is: What sort of consciousness accompanies the conduct, thus characterized, of the animal who learns by experience? As parallel of the preliminary, random performances, there is no need to assume any save a sensational (and primitively affective) consciousness of the animal's environment and of its own movements. But what is the nature of the consciousness which accompanies the immediately successful performance, from which these instinctive, random movements have fallen away, so that perception of environment is at once followed by the acquired reaction? Psychologists are agreed, in the first place, that these acquired reactions, gained by repeated satisfactions, require imagination on the part of the animals who learn to perform them. The rat which unerringly makes the successive right turnings through the labyrinth, in which at first, he ran about ineffectively, is evidently guided by an image of the food which he has repeatedly found at its centre. (The possibility that the mere smell of the food sets off purely motor reflexes, whose success is due to habit—that the rat, in other words, does not imagine the food but actually smells it—is excluded by experimenting in a fresh labyrinth without food at its centre. In this case, the rat's movement evidently is not initiated by an external stimulus.)

The significance of imagination to animals who learn by experience is, indeed, admitted by practically all psychologists. Thorndike alone minimises its importance; but even Thorndike holds that he has proved imagination in the mental life of his cats[1]. These animals, who had repeatedly heard him speak a sentence and had then seen him rise and give them food, learned to climb up for the food on hearing his voice—that is, before they saw the food[2]. Of course it does not follow that animals remember in the sense of referring experiences to their own past life, of reflectively dating them, as it were. It is unlikely that such relational consciousness occurs, and in any case it is impossible, with our necessarily objective methods, to prove its presence.

This consideration leads at once to a discussion of the most disputed problems of this analytic section of comparative psychology. It has just been shown that animals possess the simplest form of the learning consciousness. Do they, in addition, learn by analytical reasoning, that is, have they relational, as well as sensational consciousness? And secondly, have they the consciousness characteristic of the social type of learning: that is, do they reflectively imitate or oppose themselves to other selves[1].

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[1] "Animal Intelligence," p. 65.

[2] For confirmation, cf. Kinnaman's accounts of experiments on monkeys (*Amer. Jour. of Psychol.* XIII. p. 129). Cf. also Hobhouse, *op. cit.* p. 229 *et al.*: "If the direct way into a room is barred, a dog or cat will at once betake itself to any other route.... This adoption of alternatives suggests that their action is to be referred not to an impulse urging them to move in a particular direction but rather to a desire to be in a particular place." Hobhouse gives experimental evidence of the occurrence of these images of places.

As regards the question of relational consciousness, it is probable that animals of every order—primates to infusorians—do sometimes perform acts which might conceivably be performed through analytical, relational consciousness. To take an example from among unicellular organisms: the movement of the Stentor when it reacts to a persisting harmful stimulus, by giving up its normal position, that is, by abandoning completely its tube and swimming away to form a new one, might be regarded as performing this movement by analyzing out the relational elements of the situation—by reflecting "this stimulus recurs perpetually; to be unharmed by it I must avoid it." But the fact that an act is such as might have been performed through analytical consciousness does not prove that it was thus performed. The fact, for example, that animals react in a similar way to distinct, yet similar, stimuli does not prove that they have analyzed the stimulus and that they regard similarity as one of its elements. On the other hand, it is far more likely that they react to the situation as a whole, noticing neither its difference nor its similarity, as compared with the one which has preceded. So also, the use of implements, noted by many observers[2] in vertebrates, by Wasmann[3] and others in ants, and by the Peckhams in wasps[4], though it might be carried out through analysis of implement and of situation—this latch to be opened by this supple wire, this nest surface to be pounded by this hard pebble—is not, in all probability, due to such analysis. For it is a matter of experimental proof that even the higher vertebrates are incapable of such analysis, except in the most limited way. To a degree, they presumably learn to discriminate, else they would probably not react to more and more limited portions of their environment, but the discrimination must be of relatively concrete parts of total situations, and the analysis into abstract elements is doubtless beyond them.

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[1] Cf. p. 266, above.

[2] Cf. Romanes, *Animal Intelligence*, pp. 466, 481, 490.

[3] *Das Seelenleben der Ameisen*, Breisgau, 1900, S. 83.

[4] "Instincts and Habits of the Solitary Wasps," *Bulletin of Wisconsin Geological and Natural History Survey*, p. 22, 1898.

There is a twofold proof of the incapacity of animals for abstract analysis. In the first place, the apparent results, in the animals' behaviour, of an analytic consciousness, often lack the permanence of conduct due to the truly analytic consciousness. Thorndike's animals, for example, after six or seven times performing some mechanical operation, would "for ever after fail to do it[1]." This persistent failure would have been impossible, had the animals initially succeeded through analysis of the situation and through consequent discovery of the precise adjustment of means to end; for such analysis and the resulting consciousness of relation are not readily forgotten. On the other hand, the failure to repeat the successful movement is comparatively natural, if learning is due to the chance connexion of total situation with total movement.

The incompleteness of an animal's analyzing consciousness is shown, in the second place, by the gradual advance which it makes in learning. The complete analysis of situations, such as is involved in solving problems and in guessing riddles, for example—is normally achieved instantaneously: it is a sudden flash and no gradual dawn of light[2]. Therefore the generally regular progress in an animal's learning, like the evanescence of the results of its experience, tends to discredit the significance of analysis in the animal consciousness. But the consciousness of relation certainly requires a high degree of capacity to analyze: the elements of similarity, difference, connexion and totality are with difficulty held by the attention. The fact that animals, to so slight a degree, analyze their experience is a strong indication, therefore, that they do not have the consciousness of relations. The presence of a slight degree of relational consciousness, nearly swamped by concrete experiences is, however, not to be disproved by objective tests. Certain acts of animals and of young children, reported by trustworthy observers are, indeed, most naturally thus interpreted. Such are the cases of Hachet-Souplet's *coati*, which reached the eggs on a high shelf by drawing up a distant chair, with a scarf twisted around its legs; and that of the child of fourteen months who was observed "to feel his own ears, and then his mother's, one day, when looking at pictures of rabbits[1]." In the main, however, it is fairly certain that animals react to situations as wholes, that they seldom or never analyze, that they consequently lack relational experience—which, of course, means that they reason, if at all, from concrete to concrete[2].

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[1] *Psychological Review*, Vol. v. p. 552, and "Animal Intelligence," p. 44. Cf. also "The Mental Life of Monkeys" (p. 15) for the account of a monkey who "learned to pull a loop of wire off from a nail," and who "failed thereafter to pull off a similar loop made of string."

[2] Cf. Lindley, "A Study of Puzzles," *Amer. Jour. of Psychology*, Vol. VIII. pp. 478 *seq.*, 1897.

The second fundamental disagreement among students of animal psychology concerns the problem: do animals learn by the second sort of learning consciousness; do they, in other words, possess the social consciousness involved in reflective imitation and opposition? The main reason urged by those who contend that animals do not imitate, in the social sense, is that imitation and its converse—opposition—involve the consciousness of oneself in relation to other selves, and that animals are incapable of self-consciousness. In the opinion of the writer, this position is entirely untenable. Animals, if they are conscious at all, must be conscious of selves, for consciousness of any other sort is inconceivable. To be conscious simply means to be conscious of oneself in this or that or the other situation. The only ground for questioning this view is, in truth, the old tendency to confuse the implicit self-consciousness of every experience, with the definite, discriminated, reflective self-consciousness of the psychologist or the philosopher. Self-consciousness, in the latter sense, is as impossible to the animal as to the child, and is properly opposed by the ordinary argument: infants and animals, because incapable of abstraction are therefore incapable of self-consciousness. Self-consciousness as a vague, undifferentiated sense of what Hobhouse calls "self as a pervading identity and permanent character[3]," every animal which is conscious at all must possess[4]. Indeed, all who admit, as most psychologists do, that animals possess the primal emotions of affection and aversion—probably, also, of sympathy and of jealousy—thereby grant the self-consciousness of animals.

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[1] F. Tracy, *The Psychology of Childhood*, Boston, 1893, p. 44.

[2] For discussion of the nature and forms of reasoning, cf. the writer's *An Introduction to Psychology*, pp. 240 *seq*.

[3] *Op. cit.* p. 312. Cf. C. L. Herrick, "The Beginnings of Social Reaction in Man and Lower Animals," *Journal of Comparative Neurology and Comparative Psychology*, Vol. XIV. pp. 118 *seq*., 1904.

[4] Cf. Hachet-Souplet, *Examen psychologique des Animaux*, p. 81, "Un chien est assis sur un banc, je crie: 'Ici!' il vient immédiatement. Six chiens se trouvent assis sur un banc, et, parmi eux il a pris place le premier.... Je crie: 'Ici!' sur le même ton que précédemment; aucun ne bouge. Chacun attend donc que je dise Dick! Tom! ou Pompon!... Ils savent qu'il existe d'autres chiens qu'eux. Cela prouve clairement que le chien a la notion de sa personnalité."

It does not, however, follow that the self-consciousness of animals is either as explicit or as complex as the self-consciousness of adult human beings. In particular, it is not proved that any animals have attained the explicit self-consciousness involved in reflective imitation. The great majority of cases of alleged animal imitation—for example, the imitations involved in the so-called social life of insects[1]—fall into two classes: they are either fortuitous repetitions of actions instinctive in a large group of animals—and, in this case, they are not, in any true sense, imitations—or they are merely mechanical imitations, that is, imitations known as such by the observers not by the performers, in which the action of one animal serves as the direct stimulus of another animal's act. Reflective imitation, on the other hand, involves the purpose of the imitator to model his own act on that of another self[2]. Among recorded cases of animal imitation, Hachet-Souplet's record of dogs who imitate the trainer's voice[3], and Kinnaman's account[4] of his monkey's imitation of its fellow, represent what seems most like reflective imitation; yet neither case is decisive. An objective test of imitation of this sort, and of the correlated experience—reflective opposition to other selves—would, however, be so hard to find, that it is not fair to deduce merely from the lack of proof the positive impossibility of the experience. It is still less justifiable to base an argument against the occurrence of animal imitation on the fact, noted by Thorndike[5], that animals fail, often, to perform actions which the experimenter or a fellow animal has repeatedly performed in their presence. For in these cases, it is likely that attention, certainly a prerequisite to imitation, is absent.

The discussion of the scope of child psychology, were there time to introduce it, would follow a similar plan. The first question of child psychology would, however, differ from the initial problem of animal psychology. There would be no reason to ask: What children are conscious? For there is here no question of different species, but only of different individuals. In other words, it is generally admitted that all normal human beings, at some period of their development, become capable of both sorts of learning through individual experience: first, learning by trial and error, in dealing with concrete situations; second, learning by analysis of these same situations. It is admitted, also, that all normal human beings develop, in the social fashion, by reflective imitation and opposition, both concrete and analytic. The uncertainties of child psychology, it may be noted,—the problem, for example, of the age at which a child first reasons, and the problem of the origin, spontaneous or imitative, of the child's language—belong mainly to the period of infancy when the comparative method alone must be employed, that is, when the nature of the child's consciousness must be established by objective tests. The more important domain of child psychology is, on the other hand, the study—supplemented by the introspection of the older child—of its more developed processes of thought, of its realized imitations of other people, and of its self-assertive oppositions to them.

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[1] Cf. Wasmann, *op. cit.* Kap. I.

[2] For development of the doctrine that imitation is primarily personal, cf. J. Royce, *Century Magazine*, 1894; also, the writer's *An Introduction to Psychology*, p. 341.

[3] *Op. cit.* p. 106.

[4] *Op. cit.*, *Amer. Jour. of Psychol.* XIII. pp. 122 and 199.

[5] "Animal Intelligence," pp. 60—62; "The Mental Life of Monkeys," pp. 32—42. Cf. Hobhouse, *op. cit.* pp. 148 *seq.*

This address has aimed to suggest the scope and the limits of genetic and of comparative psychology. In particular, it has attempted: first, to vindicate the right to existence of genetic psychology—viewed as a science of developing selves; and, second, to formulate and to apply a safe, objective test for the comparative study of consciousness. Incidentally, this paper can hardly have failed to show, that the experimental method, already so fruitful in psychology, may with advantage be applied, also, to the solution of genetic and of comparative problems; provided, first, that it be based on sound conceptions, and second, that it be applied in accordance with the claims of a rigid logic.

ANALYSIS OF 'LOCALISATION,' ILLUSTRATED BY A BROWN-SÉQUARD CASE.

By C. SPEARMAN.

§ 1. *Present obscurity concerning the 'localising' process. Elucidation hoped from a remarkable 'Brown-Séquard' case recently admitted into the Nerve-hospital at Leipzig.*

§ 2. *History of the patient. Surgical operation. Prolonged series of tests.*

§ 3. *His movement-sensations are found almost extinct in part of the left leg, but elsewhere intact.*

§ 4. *His contact sensibility, on the contrary, is more degraded in the right leg.*

§ 5. *His power of localisation proves incompatible with the extreme movement theory. Curious effects of fatigue. Some apparent contradictions.*

§ 6. *Analysis of the 'simple' localising procedure: two kinds of sensory messages; their physiological summation.*

§ 7. *Analysis of the 'looking' procedure: a second independent means of localisation. 'Allocheiria.'*

§ 8. *Analysis of the 'groping' procedure.*

§ 9. *Analysis of the procedure with compasses; a third independent means of localisation. Some experiments. Evolutional explanation. Aubert and Kammler.*

§ 10. *Criticism of Förster's recent work.*

§ 11. *Summary of conclusions. Tables. Diagrams.*

§ 1. *Introductory.* It would be superfluous to dwell upon the psychological and philosophical importance of the problem, as to how we manage to feel the position of our limbs; or, more precisely, as to how our tactile sensations come to be endowed with 'thereness.' The earliest systematic investigation into the matter seems to have been that of E. H. Weber in 1834[1]; of his results—which caused no little stir in the scientific world—the one most highly prized by himself[2] was the demonstration of a wide gulf parting all the more subjective sensibilities from this seemingly objective perception; for while the discrimination of pressure, heat and cold was found to be nearly equal all over the body, that of space proved to be no less than 50 times finer in some places than in others[3]. Thirty years later the distinction was corroborated and further evolved on the pathological side: Leyden discovered that disturbance of the localising power, though on the whole proportional to that of the other sensibilities, could sometimes be almost independent[4].

Modern opinion has diverged from Leyden's, but in two contrary directions. On the one hand, a number of workers have drifted back into the old complete identification of the sense of space with that of simple contact; thus Laehr[5], from a careful study of sixty cases of *tabes*, is able to declare that "the lightest injuries to the tactile sensibility are those in which touches can still be quite well felt, but are falsely localised"; he even states that dulness to contact and inaccuracy of localisation often appear merely alternating symptoms in the same area of skin. On the other hand, vigorous efforts have been made to sunder the two functions still further, deriving the sense of space more or less completely from another factor, movement. As an account of the ultimate origin of 'thereness' in ancestral or infantile experience this theory is ancient enough[6]; but there has arisen a tendency to apply it to adult persons in their present condition. Accordingly, in 1901, Förster[7] formally declared that the localising power is not at all dependent upon contact sensibility, but wholly so upon 'movement sensations' (*Bewegungsempfindungen*). For proof he brings 19 varied and carefully examined pathological cases from his own experience; as regards previous medical records he says that they are all on his side, if only inspected critically enough. He admits that in the class of cases named after Brown-Séquard the injured localisation seems—according to the text-books—to be accompanied by insensibility to contact rather than to movement; but he points out that very few instances of this kind have been adequately investigated, and that never has the alleged loss of localising power been measured *quantitatively*, so that these very isolated exceptions may well be only errors of observation.

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[1] *Annotationes anatom. et physiol.*

[2] See *Berichte d. k. Gesellschaft d. Wiss. zu Leipzig*, M. P. Klasse, 1852, S. 86.

[3] Elsewhere given by him as 60 times (*Die Lehre vom Tastsinne u. Gemeingefühle*, 1851, S. 78–9): *a priori*, the greater uniformity might have been expected from the spatial perception.

[4] *Virchow's Archiv*, Bd. XLVII. S. 321.

[5] *Archiv für Psychiatrie*, Bd. XXVII. S. 693 and 725.

[6] See the works of Thomas Brown, the Mills, Steinbuch, Bain, Krause, Spencer and others; Wundt's position is less extreme.

[7] *Monatschrift f. Psychiatrie u. Neurologie*, Bd. IX. S. 31 and 131.

There is another fundamental question which has given rise to no less difference of opinion, namely, whether this so-called spatial sense be not really a confusion of two distinct elements. As regards the great bulk of Weber's work, the reagent was simultaneously touched by both compasses-points, which were gradually neared until the exact distance was found where they could only just be distinguished; occasionally, however, the variation was introduced of touching the reagent in one place only and letting him try to indicate the spot (without looking). But in 1858 a protest was raised, Aubert and Kammler[1] declaring that the compasses tested nothing but the pure 'sense of space' (*Raumsinn*) into which our outer sensations range themselves by transcendental necessity, whereas the spot-finding method dealt with the very different 'sense of locality' (*Ortsinn*) elaborated out of the spatial sense by long experience of movements. A step even further was taken by Brown-Séquard[2], who reduced the compasses to the rôle of a 'barometer of the state of the skin' and tested localisation exclusively by spot-finding. But next Leyden[3] came forward as a trenchant champion of the older view; once more the spatial sense was regarded as one single function and all its various manifestations were held to be most completely and satisfactorily tested by the compasses alone. Nor to this day has any general reconciliation been achieved; the distinction between the sense of space and that of locality continues to be maintained and denied with unabated energy—and, perhaps, with increased acrimony.

The present paper is an acceptance of Förster's challenge; a particularly unequivocal Brown-Séquard case has been taken in hand, and the patient's power of localisation has been submitted to the careful quantitative measurement not unreasonably demanded: upon this occasion the same rigorous procedure has also been applied to the other two terms compared, the senses of contact and of movement (as far as I am aware these three faculties have never before been quantitatively tested in any one person). Moreover, the localising faculty has been tried by both the methods of Weber, as well as by the principal other types of procedure, the results arrived at being often of instructive discrepancy. By the aid of the information thus attained an attempt is made, not only to clear up the two long vexed questions mentioned above, but also to attain a clearer and deeper analysis of the whole localising process.

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[1] Moleschott's *Untersuchungen*, Bd. v. 1, S. 174. See also Aubert's *Physiologie der Netzhaut*, 1865, S. 6–10.

[2] *Journal de Physiologie de l'homme et des animaux*, 1858.

[3] *Virchow's Archiv*, Bd. xxxi. S. 1 and 2.

To Professor Flechsig, Director of the Leipzig Nerve-hospital, I am indebted for his most kind permission to examine the patient freely, and further, for his invaluable personal assistance. I must also express my hearty thanks to Dr Kaulfers, who was in special charge of the case and was ever ready with his useful cooperation.

§ 2. *History of the patient.* To avoid the not uncommon confusion between observation and hypothesis I will narrate the facts of the case as simply as possible first, then proceeding to analysis and suggested interpretation.

On the 12th of September, 1903, S., a powerfully built miner, 45 years old, of healthy and well-nourished appearance, presented himself at the hospital, where the following symptoms were recorded. *Motor:* walk is spastic-paretic; there is a general restriction of movement and reduction of strength in both legs, but much more markedly in the left. *Kinaesthetic:* in the left leg all movements are atactic and the patient cannot tell what position has been given to it; in the right, the ataxia is slight and perception of position is normal. *Sensory:* almost all the lower part of the body has become insensible to touch, pain, temperature and vibration; the upper limits of the anaesthetic zone are sharply defined by a horizontal girdle about 10 cm. above the navel; on the left side there is considerable improvement lower down; but on the right only the toes and sole still have some slight sensibility. No sweat is excreted in the anaesthetic zone, although abundant in the upper portions of the body; the patient has lost all sexual power and suffers from difficulty of defecation and urination.

Thus S.'s left leg could hardly move, while his right could scarcely feel. The physician perceived that the symptoms were of the Brown-Séquard type and accordingly suspected one-sided lesion of the spinal cord; Röntgen rays were turned on and actually revealed a small triangular foreign body to the left of the 6th dorsal vertebra. Thereupon the patient was induced to relate that twenty-six years previously he had been stabbed in the back; for some months afterwards he had been unable to use his left arm, but since then had not been further troubled; he had no idea that the point of the weapon had broken off inside. He said that the present symptoms had gradually commenced about two and a-half years ago; first, there had merely been a weakness in the left foot; this had spread up the leg; and all the other mischief had soon followed. An operation was performed upon him on the 5th October; pressing against, but not severing, the dura mater on the right side of the spine, was found the anticipated rusty old knife-point, which was easily extracted. The immediate effect of this operation was to aggravate the symptoms generally; but then commenced a steady though very slow improvement.

My own examinations of the patient extended from May to August, 1904; such a long period enabled me to give him an adequate methodological training and to apply sufficiently numerous tests (about 3800). Thus, aided by the patient's great willingness and intelligence, I was in a position to obtain data which are, I believe, of unusual reliability[1]. The results will be given under the three principal heads: movement-sensations, sensations of contact and power of localisation.

§ 3. *Movement-sensations.* By this term will be understood that whole system of sensations which constitute or immediately engender our feeling that a limb moves[2]. Strictly speaking, the present tests only dealt with movements of a passive nature[3]; but this qualification will hereinafter be omitted, chiefly for brevity's sake, but also because the perception of active movements only differs in introducing some additional factors. The precise anatomical seat of these sensations—variously attributed to the muscles, skin, joints, or tendons—need not here be discussed; their existence, at any rate, is indisputable.

To excite them alone is not a very easy matter. Leyden[4] slung the tested leg from a high pulley on a gallows-like structure; Goldscheider[5] worked it up and down by means of a pump in the room underneath, breaking through the floor for the purpose; the Americans seem usually to put a hinged board under the limb. But all these devices, apart from other inconvenience, suffer from the disadvantage that the movement of the supporting apparatus does not at all accurately coincide with the direction of the rotation of the limb; hence, the reagent's skin is subjected to very disturbing tensions and frictions. To escape these a large wheel was fixed for me upon a stand and furnished with a laterally projecting well-padded board; the limb to be tested is placed upon this board in such a manner that the axis of the joint is in prolongation of the axis of the wheel; then the wheel—and with it the limb—can easily be rotated at any desired speed; 1 degree per second is employed mostly.

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[1] Data, such as are ordinarily to be obtained in the sick-room, would in any good psychological laboratory be taken as mere preliminary exercise.

[2] Sometimes this expression is used to denote, not sensations revealing movement, but those caused by movement; more often, unfortunately, these two meanings are confounded. In the present case there is little fear of ambiguity, for the two phenomena practically coincide, in that the sensations caused by a limb's movement always bring with them a perception of this movement; the only exception is for those minimal changes of position which are just enough to produce sensation, but yet insufficient to excite idea of motion (Goldscheider, *Archiv für Physiologie,* 1889, S. 373).

[3] That is, movements imparted to a limb by another person.

[4] *Virchow's Archiv*, Bd. XLVII. S. 344.

[5] *Loc. cit.* S. 396.

With this apparatus, S.'s right leg could just distinguish upward from downward movements[1] when these were of the following amplitude: about the hip, 0·7°; about the knee, 0·8°; about the ankle, 1·2°; these amounts are quite normal. But with the left leg things were very different; the hip was again normal enough; but with the knee a rotation as large as 20° could not be noticed; while the ankle failed to yield any sensation whatever from 40° (the greatest amplitude I could produce). Now, it happened that two or three times a week S. was allowed a few minutes' walking exercise, which appeared to greatly fatigue him; on testing him in this state the right side showed itself still unaffected; but on the left even the hip had become nearly insensible, the knee totally so, while the ankle was too stiff to be tested at all.

It will be observed throughout that the transition from soundness to infirmity is demarcated with remarkable distinctness; a joint either escapes altogether, or else it is affected profoundly; and even a temporary fatigue can at once transform the one state into the other. This may be taken as indicating that the fibres transmitting these movement-sensations to the brain are, at any rate near the 6th dorsal vertebra, ordered into very compact groups corresponding to the respective joints[2]; a similarly compact grouping seems to exist in the cortex also, to judge from the recorded effects of lesions in the central and parietal convolutions[1]. In spite of obscure origin, the movement-sensations of a joint appear to form a very well-defined physiological unit, and therefore can hardly originate from several substantially independent sources, as often supposed.

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[1] Goldscheider conjectured that the faculty of discriminating a movement's direction might differ from that of noticing the mere fact of movement. No such discrepancy was noticed here; the stimulus necessary for the latter purpose was smaller than, but always proportional to, that required for the former. As discrimination of direction is far less disturbed by irrelevant factors it will be treated as the paramount criterion.

[2] The more peripheral parts of the leg are apparently represented by more lateral situations in the spinal cord.

§ 4. *Contact sensibility.* For testing this recourse was had to von Frey's useful 'hair method[2].' A hair is pushed against the skin until the former begins to double up and lose force; the shorter the hair is made the stiffer it becomes and the deeper is the impression on the skin; the length at which the patient can just perceive that he is being touched furnishes the required measure of sensibility. I find it convenient to insert the hair in an ordinary pencil-case (by means of a minute cork sheath), so that the end can easily be projected to any length desired. My only essential modification of von Frey's procedure concerns the hair's tip, to which I attach a tiny globule of glue, in order to make it smooth and spherical; this is important, seeing that otherwise the bending hair has continually different amounts and shapes of surface in contact with the skin, and the sensory effect must be always changing[3]. Tested in this manner S. presented none of the contrasts just discovered for the movement-sensations. Some degree of abnormal insensibility was found all over both legs, especially the thighs and toes; when he was not tired, the right leg was only slightly worse than the left; but in fatigue, the former further deteriorated by about 50 °/₀, the latter by about 20 °/₀.

The senses of heat, cold, and pain were only tested in the usual rough unquantitative manner; all three seemed fairly normal in the left leg, but could hardly be elicited at all in the right (although the sensation of cold arose here *subjectively* so much as to annoy the patient, even when the rest of his body was unpleasantly warm).

§ 5. *Power of localisation.* Everything is now prepared for a crucial investigation of this faculty. On S.'s right side all the contact sensibilities are greatly degraded, while the movement-sensations remain perfectly preserved; but on his left he can still feel contact tolerably, while to movement he is almost wholly insensible. If localisation depend upon perception of movement it must prove vitally injured in his left leg, but unimpaired in the right; if, on the contrary, the localisation closely correspond with the sense of contact, then we must expect to find it deteriorated in both limbs, but more so on the right, especially when S. is fatigued.

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[1] Dana, *Journal of Nervous and Mental Diseases*, 1894; Wernicke, *Arbeiten a. d. psychiat. Klinik in Breslau*, Heft II.; Starr, *Psych. Review*, Vol. II.; Frenkel, *Neurolog. Centralblatt*, 1897, S. 688 and 734.

[2] *Berichte d. k. sächs. Gesell. d. Wiss.*, M.P. Klasse, Bde. XLVI. and XLVII.; also *Abhandl. d. k. sächs. Gesell. d. Wiss.*, M. P. Klasse, 1897. The sensibility of the tactile organs to this hair contact seems to correspond with that to electricity, but not with power to discriminate pressure or strain.

[3] The sensory effect was found by von Frey himself to $= k/d$, where k is the maximum force of the hair and d is the diameter of the contact surface (*Berichte d. k. sächs. Gesell. d. Wiss.*, M. P. Klasse, Bd. XLVI.).

We have seen that there is a choice between two testing methods, that of distinguishing the compasses' points, and that of indicating the position of a spot touched. The latter must be further subdivided into at least three different procedures. First, there is the one used by Weber himself[1]; the reagent is not allowed to open his eyes before pronouncing judgment, but may freely grope about for the place. Then there is Volkmann's procedure[2], by which looking is permitted, but not groping. Finally, both looking and groping may be forbidden[3]. For brevity, these three procedures of spot-finding localisation will be termed 'groping,' 'looking,' and 'simple' respectively.

Commencement was made with the simple procedure, for reasons which will appear later. It was conducted by me as follows. After closing the reagent's eyes, the limb to be tested is moved about irregularly until his last view of it can no longer help him; to prevent tactual recognition and correction, the limb is shielded at 1 cm. distance by a sheet of cardboard covered with white paper and perforated in the centre by a small hole; through the hole is introduced a bent wire 2 mm. thick and cased in cloth (to avoid cold). The reagent is given a stylographic pen and, on the wire pressing his skin, endeavours to indicate the corresponding spot on the cardboard[4]. Thus is immediately produced a graphic representation of the errors, showing both size and direction. As a rule it is important in this sort of test to distinguish the variable from the constant error; the former alone is the subject of the present work; but it may here be mentioned that S.'s constant errors happened to be so small as to be in any case almost negligible. The results by this procedure seemed as conclusive as could be desired. From the left knee downwards—where we had ascertained the contact sense to be injured moderately, but the movement-sensations to be almost wholly destroyed—there the patient now showed himself to have also lost all power of recognising the position of touches; the median error was no less than 10 to 17 cm., which comes to mere random guessing. But on the left thigh and whole right leg—where the injury to the contact sense had proved greater, but the movement-sensations were perfectly normal—there the error was about 4 cm., an amount no larger than often occurs with healthy persons (average, a little over 3 cm.). The only sign of localisation corresponding with sensibility to contact was that the left thigh did slightly better than the right (4·1, as against 4·8); to obtain more decisive evidence all the tests were several times repeated when S. was tired; in this state, though his contact was greatly reduced, his error of localisation never showed appreciable change anywhere. So far the evidence sufficiently agrees with Förster's view, that the power of localisation depends entirely on the movement-sensations and not at all upon the sensibility to contact.

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[1] *Berichte d. k. sächs. Gesell. d. Wiss.*, M. P. Klasse, 1852, S. 87.

[2] Wagner's *Handwörterbuch der Physiologie*, 1845, S. 571–2.

[3] This was done in some experiments of Henri (*Über die Raumwahrnehmung des Tastsinnes*, 1898, S. 107), but his manner of procedure differed widely from that adopted by myself.

[4] The reader will naturally suspect that a new factor may be introduced in the fact that the reagent has not got to indicate the touched spot itself but another spot just above. But some extensive experiments (not yet published) have shown this [illegible] make no appreciable difference.

We turn to Volkmann's visual procedure. The reagent, after noting the position of his limb (which is not subsequently shifted) shuts his eyes; some selected spot is touched for one second; a pause is given long enough for the mark on the skin to die away[1]; then he is told to open his eyes and point out the place, but without touching it. The previous remarkable correspondence of localisation with movement proved now to have abruptly vanished. The formerly much inferior left leg had become even the better of the two, its thigh had an error of only 3·8, as against 6·2 on the right (normal, about 2·0); the calf showed 3·7, as against 4·1 (normal, about 1·8); the feet were perfectly equal on both sides, with an error of 1·8 (normal, about 1·5); only on the toes was the left leg still at a small but steady disadvantage, being 1·7, as against 1·4 (normal, 0·6). All these tests were repeated in S.'s fatigued state, and all the errors again remained unaltered. *Such performances do not seem possibly reconcilable with the extreme theory of movement-sensations;* for instance, these sensations have been found almost extinct in S.'s left knee, but perfectly intact in his right knee; yet the calf on the former side unquestionably localised the better of the two. As regards influence of skin sensibility, the evidence seems contradictory; for though the localising error is generally greater on the parts duller to contact, sometimes it is even less on these parts.

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[1] Reagents generally think that a delay of two or three seconds greatly affects their power of finding the spot, but objective measurements show no appreciable falling off.

We come now to Weber's own procedure, where the reagent's eyes must not be opened, but he is allowed to grope for the spot. S. was allowed to use his finger-tip for the purpose[1]. The remainder of the procedure is just the same as in Volkmann's, except that now there is no need to wait for the mark to disappear. The results entirely corroborated the above visual ones; again, the left leg was superior in the thigh and calf, equal in the foot, and inferior in the toes; once more, no change took place during fatigue. For completeness, S. was occasionally allowed to both look and grope. One might expect such a combined performance to exhibit increased accuracy, but in practice I have always found it merely give values intermediate between those obtained by using either clue separately. This proved to be the case with S. also, so that the results do not appear interesting and will not be further mentioned.

Finally, we pass to the other general method of testing localisation, that with the compasses. It was first executed in the ordinary manner; that is to say, both points were placed upon the skin simultaneously[2]. As result, the right leg continued to be slightly better than the other; when untired, both showed great dulness on the thighs, but elsewhere gave nearly normal values; *after exercise*, however, the whole lower part of the body became so insensible, that the two points could not be distinguished at any distance whatever (on the same segment, of course). Long ago it was pointed out that duality of sensation and spatial separateness are not identical; if, for instance, a hot and a cold point be applied to the skin within the same sensory area, the two sensations remain qualitatively distinct, but do not appear any more spatially separate than do two simultaneous musical tones[3]. I therefore modified the previous procedure as follows: I touched the patient with one point only, ceased doing so for one second, and then touched with the other point; he had to decide whether the second stimulus was higher up or lower down than the first one. But even then, although the task had become purely spatial—and at first sight might seem almost identical with Weber's spot-finding—the objective facts remained quite unchanged; when untired, S. still gave fairly normal values everywhere except in the thighs; after his few minutes' exercise, he still appeared to have lost all measurable discrimination. *Thus the influence of the movement-sensations has now wholly disappeared, while that of skin sensibility would seem contradictory.*

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[1] Most reagents prefer this, but their actual performances are no better than when using a wire or stick.

[2] In this sort of work I do not try to make the reagent examine subjectively whether he can detect a doubleness of sensation, but I test his practical power of deciding rightly as to whether he has really been touched in one or on two places.

[3] Czermak, *Berichte d. k. Akad. d. Wiss. zu Wien*, M. N. Klasse, Bd. xv. 1855, S. 500.

So much for the measurements taken of the patient's sensibility. Now comes the very different task of interpreting them. In the above tests as to power of spot-finding, equal consideration has been allotted to three procedures, 'simple,' 'looking,' and 'groping' respectively. This, however, is by no means in accordance with traditional usage; for while dozens of laborious researches have been devoted to the two latter, the first sort has hitherto been almost wholly ignored[1]. This appears to me as beginning at the wrong end; the 'groping' and 'looking' procedures can scarcely be treated otherwise than superficially, until a thorough investigation has first been made of the 'simple' one.

§ 6. *Analysis of 'simple' localisation.* Let us try to analyse the process; suppose an experimenter to touch a reagent on the foot; then the reagent, without looking, has to bring down his finger-tip until it seems to be just opposite the spot. But how can he know when this is the case? Seemingly, the mind flashes straight across from the sensation in the finger to that in the foot; but this could not really occur unless our limbs were able to perceive space objectively, a proposition that the most ardent realist would hardly dare to assert nowadays. Failing this short road to cognition, we are compelled to take one very roundabout; the brain cannot perceive that the finger is hovering near the touched spot, unless receiving sensory messages from all along the circuitous bodily connexion between the two; just as the pioneer cannot know that his tunnel is nearing its goal, unless maintaining a roundabout real connexion with the latter by a chain of back bearings.

The above requisite messages must necessarily be conceived as belonging to two different classes. First, there must be the excitations[2] which reveal the inclinatory angles of all the intervening joints; in our above example the reagent must at least be undergoing such excita-

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[1] The only notice of it appears to be the previously quoted one of Henri, and even there it does not occupy a fiftieth part of his whole work.

[2] The term 'excitation' is chosen as most neutral. The reason for this caution will soon be evident.

tions at the finger joints, wrist, elbow, spine, hip, and ankle. And secondly, there must be the excitations at both ends of this open circuit, revealing the positions of the two intentionally compared parts with reference to their own anatomical segment[1]; in the case we have just supposed the reagent needs to know the position of the tip[2] with reference to the finger and that of the touched spot with reference to the foot. These two classes of sensory messages are so important and must be so often cited as to urgently require brief names; the difficulty is to find any that do not prejudge some question under dispute; the excitations varying with the angle at the joint will be termed 'articular,' while those depending only upon the rigid segment between joints will be called 'segmental.'

Now, what relation do these two logically necessary messages bear to our actual sensations? For the segmental messages we evidently need to go no further than the ordinary sensory activities of the skin; the touched spot on the reagent's foot knows by long experience its relative position with regard to the foot; similarly, the tingling in his finger should be perfectly well able to tell that it comes from the tip.

Less easy is it to decide what furnishes the articular messages. How do the foot and finger discover in what direction they are pointing? In some way or other these articular messages or indications must be intimately connected with the movement-sensations, seeing that the former depend upon the joint's angle during rest[3], while the latter depend upon the same angle during motion. Yet Goldscheider, embarrassed by certain difficulties (which we also shall have to encounter shortly), was driven to the hypothesis of two different sets of organs, one about the tendons and folds of the skin for the orientation during rest, and one in the joint for that during movement. But this hypothesis, already so improbable from a general point of view, becomes on closer empirical scrutiny quite untenable; medical records are unanimous in showing that loss of the movement-sensations is invariably accompanied by disorientation as to position, and *vice versâ*; in our case of S. the movement-sensations and the positional indications always behave as a single physiological unit and, indeed, as a remarkably well-defined one; its faculties act as a whole—either escaping altogether, or being profoundly injured—quite independently of the normal or abnormal state of the other tactile processes. Whether we retain an old-fashioned faith in the muscles, or whether we are led by Goldscheider to put our confidence in the joint-surfaces, or whether we accept Pillsbury's[1] demonstration of the paramount importance of the tendons, at any rate *only one* of these sources must be held mainly responsible for orientation both in rest and in motion[2].

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[1] 'Anatomical segment' is meant in a purely mechanical sense, indicating the extent of body which lies between two adjacent joints; it is nearly but not quite equivalent to Goodsir's and Huxley's word 'somatome.'

[2] If a stick be used the matter only becomes slightly more complicated.

[3] Hereby is only meant that the 'articular messages' *so far discussed* occur during rest; later on reason will be seen for extending the same term to the messages during motion also.

But this identification may be conceived in various ways. Some authors try to resolve the knowledge of a limb's position at rest entirely into a mere memory of its preceding movements. This view, however, has no real foundation beyond the general hypothesis that movement is the sole generator of spatial perception; even if the hypothesis were admissible, this would be no excuse for such a crude application of it. The factual evidence is entirely in favour of the articular excitations being at present able to orient without any movement; if we had to rely upon recollection of previous actions, in the dark the errors would so accumulate that we should quite forget where our limbs were. Far from this being the case I have usually found the error of localisation reach its maximum after very few unseen passive movements[3].

With better reason is the contrary opinion advanced, that a movement sensation is no more than a continuous succession of resting or 'positional' sensations (*Lageempfindungen*). But even this view is liable to very serious objections, springing chiefly from close subjective observation. By most people, even psychologists, the movement-sensations are more or less confused with the other neighbouring tactile sensations (principally in the skin); not only are the latter much more conspicuous, but it is through them, not the real movement-sensations, that the movement is most vividly localised. With enough practice under suitable conditions, however, the real movement-sensations can be unerringly recognised and thoroughly studied[4]. The first fact elicited is that they are exclusively localised in or about the joints; but when one tries to

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[1] *Amer. Journ. Psych.* Vol. XII. 1901.

[2] Although there may be many auxiliary and modifying influences, see 2nd footnote, p. 300.

[3] Tabical incoordination often goes on increasing for some time after the patient has closed his eyes, but this case is different and more complicated. (Spaeth, *Beiträge zur Lehre von der Tabes*, 1864.)

[4] They can be observed most clearly of all when the neighbouring skin has been rendered insensitive by anaesthetics or electricity.

feel the exact angle made by two segments, one appeals preferably to the sensations far away from the joint; if attention be directed on the joint itself, the impression as to the size of the angle becomes much vaguer; thus, the movement-sensations and the seemingly positional sensations are localised in different places. Next, the movement-sensations are found to be of an exceedingly uniform character, both in quality and intensity, so that it would be difficult to find twenty distinguishable grades (and this indistinguishability lies in the sensations themselves, not in any hindrance of the discriminative activity, as is the case with overtones, marginal vision, etc.). To afford all the required indications of position, some joints would require about 200,000 distinguishable grades[1], or three hundred times as many as there are between pitch darkness and dazzling light; thus, these sensations must be considered hopelessly inadequate for the purpose of indicating position. Indeed, no attempt is made to utilise such few grades as exist, for when the movement is very small the same sensation arises, but without evoking the least idea of movement. Further, on the movement diminishing, these sensations diminish also, and on the movement ceasing no trace of them is any longer perceptible; as they vanish whenever the limb takes up position, they cannot well be formed of positional sensations. Finally, apart from all these subjective observations, there is the convincing empirical fact, that if the skin be sufficiently numbed (with faradic electricity), the sense of position will be totally extinguished[2], while yet the movement-sensations and the perception of movement will remain unaffected; but there is no longer any perception of the movement's direction.

Thus, the 'positional' sensations do not exist[3], and even the so-called movement-sensations are unable to reveal a movement's direction; there is absolutely nothing to be found in consciousness capable of furnishing the postulated articular indications. We appear forced to take refuge

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[1] To get an approximate evaluation, we can conceive the whole cone comprising all possible inclinations of the limb as consisting of part-cones whose bases have diameters corresponding to just distinguishable differences of position; similarly, the average whole possible rotation of the limb is conceived as consisting of just distinguishable part-rotations; then, the number of part-cones multiplied by the number of part-rotations gives the required total.

[2] Goldscheider calls the sense of position under these circumstances "geradezu vernichtet." *Zeitschrift f. klin. Medicin*, Bd. xv. S. 98 and 111.

[3] Or, what comes to the same thing, all tactile sensations are positional only in the limited sense that they possess 'thereness'; but this 'thereness' is itself, as has been shown, a product of many factors, including the sought articular indications.

in the hypothesis of articular indications *not in consciousness.* I would suggest that the process of tactile localisation is as follows. At every different angle assumed by a joint there occurs a correspondingly different excitation of certain tactile organs in or near it[1]; during rest, these excitations do not reach consciousness; on movement they produce sensations indeed, but not such as could enable us to recognise position; in both cases alike they produce a physiological but not a psychical articular indication. Then this indication has to combine with those from the other joints and with the segmental ones; the whole circuit of articular and segmental indications works out its own complicated summation as unconsciously as a calculating machine; into consciousness there only shoots up the total result, a simple perception of position[2]. The reader may easily gain some conception of this view if he will ask himself (say, just after waking) the position in space of a touch on his hand or foot. He will not find himself successively reviewing and adding the angles upon which this position really depends; he need not have so much as *implicit* cognition of these angles; for all he knows at the moment, nearness of the foot may be due to flexion in the knee, hip, or spine; its sidewardness may have come from abduction of the thigh or from its rotation. In localising the exact position of the touch he simply perceives—without any geometrical premisses derived from the rest of the limb or body—that the touch is 'there.'

The chief objection to the above theory is the difficulty of admitting that mere unconscious physiological processes should be capable of entering into psychical associations; and this difficulty is enhanced by the fact that tactile localisation is very sensitive to the disturbing influence of further associations; the presence of unwonted circumstances, or even the absence of usual ones, will easily cause a touch to seem in an abnormal place. If the disturbance be prolonged the normal localisation can be modified to almost unlimited extent; for instance, Stratton[3] made the experiment of wearing glasses that turned the visual field upside down; after a few days he found that the feeling in the head, arms, and legs was learning to agree with the sight and to assume the same upside-down 'thereness'; towards the end of the experiment the tactile and visual sensations and memories grew so harmonious that the feeling of upside-downness began to wear off and everything to seem as natural in the new direction as in the old.

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[1] If these are of any known specific anatomical form they would appear to be those discovered by Vater, and, a century later, rediscovered by Pacini.

[2] It is by no means intended to assert that the act of localisation is a bare fusion of all these articular and segmental indications, but that these indications are the *least* conceivable number of underlying factors; in reality there probably occur infinite further elaborative and associative complications.

[3] *Psychological Review*, Vol. III. p. 611.

But still, such indirect reasoning appears insufficient to outweigh the above very positive evidence that *the articular excitations do not function psychically, namely, by means of differences for, or in consciousness*[1]; during rest, we have found that they produce no sensations appreciable at all; during movement, they furnished just enough sensation to show that this had not merely been hiding itself, but was really and essentially useless for localisation. Nor, perhaps, is the above objection quite as formidable as it sounds; psychical processes are in most intimate and varied interaction with purely physiological ones; there appears no cogent evidence, either empirical or *à priori*, that this interaction should not in the case of localisation take a form somewhat resembling that of the usual psychical association. Why the articular excitations should fail to become conscious is explicable easily enough; let us assume that originally they did reach the sensational layer of the cortex and that the ensuing sensations somehow evoked the 'thereness' in the simultaneous sensations from the skin; as the sequence, articular excitation—articular percept—skin percept, grew more habitual and mechanical, the middle link naturally became less attended to, less active cortically and, finally, unconscious; the uselessly indirect path is now replaced by one direct from the articular excitation to the skin percept.

In the above theory, neither has perception of movement been subordinated to that of position, nor *vice versâ*, but both are placed upon the same footing; the unchanging excitations that occur during rest evoke perception of position, while the changing excitations that occur during movement evoke perception of that movement[2]. Exceptionally, either sort of perception may lead to the other: an illusion of movement can falsify a percept of position, while conversely a very slow movement may be solely perceived through the cognitions of position before and after. As there is no importance in this distinction between the unchanging and the changing excitation, we may extend the use of the expression 'articular,' so as to include the two, both rest and motion. The reason, then, why S. cannot point out any spot touched below his left knee, is not merely because here he cannot feel movement, but because here he has been deprived of the articular excitations in general.

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[1] Hereby I wish to oppose the theory that the required differences exist in consciousness and are merely too obscure to be noticed. Wundt's more subtle theory, that sensations may be hidden by essential transformation through 'chemical synthesis,' refers to the *ultimate* genesis of spatial perception, and therefore does not come within the scope of the present article, which only attempts to unravel the most recent chapter of genesis.

[2] With the merely practical superiority of being accompanied by a certain amount of sensation and forming, therefore, a useful test.

§ 7. *Analysis of Volkmann's 'looking procedure.'* Here, the reagent opens his eyes and is able to identify directly the spot by means of a mental image aroused through the touch; *in this image we have a second manner of localisation* quite independent of the pure 'thereness' described above (p. 300); the circuit of articular excitations from the finger round to the leg is no longer necessary, the contact sense within the segment having become self-sufficient. Hence, the degradation of this sense grows more momentous; in the thighs, where the hair-test had had the greatest difficulty in eliciting sensation, now this sensation proves so perverted that the image aroused is useless; S. actually localises worse than with his eyes shut. In the same degree that the contact sense becomes more influential, damage to the articular sensibility is felt less; the left leg—despite its not knowing where it is—can now localise better than the other one.

A more remarkable fact is that this advantage of the left leg gradually diminishes towards its apex. On carefully examining the looking procedure subjectively, it will be found that the *first* spatial idea is again a 'simple' localisation (a pure 'thereness' with reference to the body and, especially, head), while the mental image only arises as an after-process, correcting and focussing the orientation already begun; so that even this kind of procedure must be to some extent dependent upon the accuracy of 'simple' localisation and, therefore, upon the efficiency of the articular excitations. The diminished degradation towards the upper part of the leg may possibly be owing to the slighter damage in the articular excitations, or to the smaller number of joints involved. It will be remembered that Mott and Sherrington[1], after severing an animal's sensory roots, always found the ensuing difficulty of movement to be greater towards the apices of the limbs; their results were subsequently corroborated by Hering[2].

However, before this or any hypothesis can be considered satisfactory, it must explain why in S.'s case the phenomenon should be so exceptionally pronounced in the *toes*; for there the left side actually localises

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[1] *Proc. R. Soc.* 1895, Vol. LVII. p. 481.

[2] *Pflüger's Archiv*, Bd. LXX. S. 559.

worse again than the right. On closer examination it was discovered that lengthwise the error was very nearly equal on the toes of both feet, but *crosswise* was no less than double as large on the left; this side clearly suffered from a greater liability to confuse one toe with another. Now, it is well known that local signs, though differentiated all over any particular anatomical segment, are by no means very dissimilar to those on other analogous segments; they are much better adapted for distinguishing different parts of the same toe than the same parts of different toes. Hence, the function of discriminating one toe from another may well be expected to seek help from other sensory organs.

Solution on these lines might seem at once offered by the circumstance that the tests on the toes cause considerable flexion of their joints and, consequently, movement-sensations; the latter may form an important auxiliary, of which S.'s left leg has been deprived. We may recall Weir Mitchell's communication[1], that when a person's fingers are irregularly laced behind his back and one of them is touched, it will be less often mistaken for another if it be moved. But I have found that this improvement only takes place if the finger be moved by the person himself, when it has obvious enough reasons; a passive movement does not seem to reduce the number of mistakes in the least degree. Similar results have been obtained by me as regards the toes also; whether flexed or not in the test, they are confused with one another to just the same amount. Hence the clue afforded by the joint in our localising procedures (where the movement is always passive) must be of a subtler nature than that alluded to by Weir Mitchell. Further, it must be such as can be equally furnished while the joint is at rest; it must not consist of the movement-sensations in particular but of the articular excitations in general.

Let us again appeal to subjective observation; if the reader will try to localise a touch upon the third or fourth toe, he will notice how the sensation simultaneously takes part in the two processes above mentioned; it quickly acquires a thereness with reference to the foot and head; and also it materialises into the mental image of the limb. These are very different things[2], the thereness being an almost purely spatial percept, whereas the image is a vignetted mental picture (at first localised very indefinitely) of the particular region of the body. But the two processes will be found to be interacting sympathetically; when, say, the 'thereness' lies towards the outer side of the foot, the image will seek the shape, and eventually situation, of the fourth or fifth toe. The same curious doubleness and interaction may be detected when localising touches on the fingers; sometimes the two sorts of perception even contend with one another, much as happens when each eye looks through a differently coloured glass.

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[1] *Injuries of Nerves and their Consequences*, 1874, Chap. VII.

[2] Even if both be ultimately visual, and even if the former be derived from the latter *originally*.

Thus our explanation with regard to the toes is that, *when the articular excitations are damaged and cease to furnish touch sensations with proper 'thereness,' in default of such prompting an originally indecisive image will be apt to settle upon the wrong segment.*

The same analysis would seem to clear up at last a little mystery now over twenty years old, that of the so-called 'Allocheiria.' In 1882 attention was drawn by Obersteiner[1] to the unaccountable fact that some patients were inclined to localise tactile sensations in precisely the corresponding point on the other side of the body; some attention was roused at the time, but so small progress was made that Gay[2], summing up in 1893, was unable to suggest any even plausible explanation[3]. Upon critical review, cases of Allocheiria appear divisible into two classes, organic and hysteric respectively; we shall consider only the former (as being less equivocal) but the same explanation will be found perfectly applicable to the hysterical kind also[4]. The actually recorded perversions of sensibility seem to have been less fixed than generally supposed; if a given patient frequently takes the right leg for the left, he will sometimes take the latter for the former, and most often, probably, he will recognise both quite correctly; nor is the transference invariably from one side to the other, for it also occurs from one to another segment on the same side; the whole phenomenon is reducible to the fact that limbs are confused with other anatomically similar ones, while nevertheless the relative position as regards the limb itself continues to be perceived with almost normal accuracy. But this is what we have found in the case of S.'s toes, and have traced to a deprivation of articular excitations. Now, authentic cases of organic Allocheiria actually resemble that of S., in being caused by lesion of the spinal cord; further, the disturbance is always greater towards the apex of the limb and especially noticeable in the toes; finally, the only common accompanying symptom is just this deprivation of the articular excitations (shown by loss of sense of position and by incoordination of movement). Moreover, if the Allocheiria is thus regularly attended by deprivation of articular excitation, the converse is no less true; Brown-Séquard[1] does not fail to mention that his patients refer touches on the still sensible side over to the other; Weir Mitchell[2] finds an illusive transference in the opposite direction; S. makes the one mistake as much as the other, on the toes frequently, on the feet sometimes, on the calves rarely. In short, this organic Allocheiria appears to be nothing more than a constant and explicable symptom of injury to the spinal tracts conducting the articular excitations; the hysterical affection is a malady of these same excitations on their arrival at the cortex.

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[1] *Brain*, Vol. IV. p. 153.

[2] *Brain*, Vol. XVI. p. 481.

[3] He concludes his article as follows: "The common sensory stimuli, finding their usual channel closed against them, are conducted upwards by the non-decussating fibres and are consequently referred to the opposite side of the body. This will probably be regarded as very fanciful, and indeed the theory is only suggested *faute de mieux*."

[4] Compare the description of it by Pierre Janet, *Neuroses et idées fixes*, 1898, Chap. VI.

§ 8. *Analysis of the procedure by 'groping.'* Theoretically, this seems quite equivalent to that of the compasses, at any rate in the latter's successive and purely spatial form described on page 295, for in this the reagent is touched somewhere and then is touched in other places nearer and nearer to the original one, until their difference of position can no longer be detected; just the same occurs in 'groping,' for here too the reagent is touched somewhere and then applies his stick to successive places, until there remains no perceptible difference of position. Practically, also, the values gained by the two methods under normal conditions appear to correspond very closely[3]. But in the present pathological case the results by 'groping' diverge widely from those by the compasses, and once more reveal the influence of the injury to the articular excitations; in this and all other respects they almost wholly coincide with the results by 'looking.'

The explanation is that, as in 'looking,' so in 'groping' the *first* indispensable phase is again a preliminary simple localisation, since this alone is responsible for the finger's original movement towards the spot; contact with the skin immediately reveals a discrepancy and the finger is accordingly shifted, but the rectification is then almost at an end; subjectively, the reagent feels that he has no longer any clear idea of the position of the spot; objectively, it will be seen that, though his first correction is generally in the right direction, his subsequent gropings are just as likely to take him wrong. The finest accuracy will be attainable when no great error has been made in the preliminary simple localisation.

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[1] *Journal de Physiologie de l'homme et des animaux*, 1863.

[2] *Injuries of Nerves and their Consequences*, Chap. VI.

[3] This correspondence has been strenuously denied, on the ground that much smaller distances are gained by 'groping' than by the compasses. But this was inevitable, seeing that the former distances are mean errors, whereas the latter are 'thresholds,' which are always much larger. One might as well maintain that a German mark is not equivalent to an English shilling because the one contains 100 pfennige and the latter only 12 pence.

Even in the second or corrective phase, the analogy between the 'groping' and 'looking' procedures is much closer than might at first sight appear, for the 'groping,' too, is really effected through the mediation of a mental image. This image differs from the other one, however, not only in having a tactual instead of visual character, but also in arising later: a reagent prepared to localise by 'looking' is no sooner stimulated than he begins to form his image, so as to be ready for comparison with the real appearance; but in 'groping' the image seems to spring from the contact sensation given by the groping stick and, thence, to develope out to the original stimulus, resuscitating this, absorbing it and indicating the way to it. The image in 'groping' is much fainter than the image in 'looking' (partly because itself tactual and less easy to distinguish from the other simultaneous tactual events); but nevertheless on close observation it can be seen to engender the same curious doubleness and contention of process, as above described for the 'looking' procedure.

§ 9. *Analysis of the method with the compasses.* Here, at last, the influence of simple localisation and, therewith, that of the articular excitations, are excluded. Consequently—whether the points be applied to S. simultaneously or successively, and whether his judgment be of duality or of direction—he now localises just as well in the limbs that have lost all sense of movement as in those that possess it intact.

Less obviously intelligible is the fact, that localisation tested in this way shows itself unhurt by the grave permanent lesions, but crippled by the insignificant fatigue; whereas in all other tests the reverse occurred, for the permanent lesions seemed to do damage and the fatigue to be quite harmless. The strong influence exerted by fatigue naturally calls to mind the much criticised investigations and results of Griesbach[1]; accepting these, S.'s insensibility after exercise could be attributed to general cerebral exhaustion. More pertinent still, perhaps, is the old observation of Leyden[2], that the '*Raumsinn*' is more affected by acute than by chronic disease; he was of opinion that the increased difficulty of tactile discrimination must lead a patient to devote more attention to the matter, and thus *in time* to regain intellectually whatever had been lost as regards crude sensation. But neither point of view attempts to elucidate the completely reversed behaviour of the localising faculty under all the other conditions of test.

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[1] *Archiv f. Hygiene*, Bd. XXIV. Tl. II. S. 124.

[2] *Virchow's Archiv*, Bd. XXXI.

The first step towards understanding the reversal of behaviour is to find out what difference of condition is essential for it. Now, the whole course of our tests has been so arranged as to eliminate gradually the unessential differences; the compasses test has been brought to the successive and purely spatial form described on page 295, while the spot-finding has been reduced to the second phase of 'groping' described on pages 305–6. We thus arrive at two processes seemingly equivalent, each consisting in touching the skin and then touching other places nearer and nearer until indistinguishable from that of the original touch. But we have further seen that in 'groping' the localisation turns out to make use of a mediating image. Is this the case with the compasses method also? On careful subjective examination, I find that here, too, an image is detectable, but playing a different rôle. In 'groping' (as in 'looking') the image is the prime factor; the later touches are merely shifted until their place in the image coincides with that of the original touch, while the quality of all these touches is not attended to; the whole process is easy and mechanical. But in the compasses test, especially when approaching the threshold, we come upon a *third manner of localisation* (in addition to those by pure 'thereness' and by images); now, the later and the original touches are compared with one another directly, attentively and, to some degree, qualitatively, the mental effort being very appreciable; the image no longer rises quite freely, but has become more or less dependent on the above intellectual comparison[1].

Though the above subjective observations have kindly been corroborated by other psychologists to whom I have appealed, I was very desirous of obtaining the more tangible evidence of experiment. It seemed that if the compasses method but not the spot-finding depended upon direct discrimination of sensation, the former but not the latter should be disturbed by confusing the quality of the sensation; I tried, therefore, the effect of making the original touch as different as possible from the later ones (ringing the changes on pointed, blunt, warm, and cold). The result in the compasses test was actually to show that the change of quality produced somewhat more mistakes in every reagent; but the difference almost always vanished with two or three minutes' practice, so that by the time the threshold had been systematically ascertained this was just the same as without any change of quality. In spot-finding, on the other hand, the change of quality made not the slightest difference from start to finish. Another series of experiments produced much more striking divergence between the two sorts of localisation; here, my reagents had the self-sacrifice to allow the stimulus to be given firmly with a sharp point, so as to be seriously and distractingly *painful*; this had the remarkable effect of *making the results with the compasses worse, but those with spot-finding much better*[1]; in the latter test, also, the act of localising seemed to acquire a more reflex character. The experiment has been repeated on several persons, always with similar results.

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[1] Of course due allowance must be made for the fact that in the compasses test the two compared touches are always in immediate succession, whereas in the course of the groping the nature of the original touch has time to become forgotten; but I cannot find that this memory factor makes any essential difference; with the compass method, even when the reagent's attention is purposely distracted and a long interval is allowed to lapse between the original and the later touches, these continue to be compared in a direct attentive manner not occurring in spot-finding.

On the strength of all these observations and experiments[2], I would suggest that the strangely reversed behaviour of the localising faculty may be explained as follows. The spot-finding, whether simple, 'looking,' or 'groping' is a procedure continually occurring in ordinary life, and has long since hardened into a fixed association between the sensations and the spatial images. But the business of distinguishing two points as close to one another as possible is new to us; no associations have been established in place of thinking. Hence, any sensory organs not injured seriously may still be active enough to evoke automatically the 'thereness' and the images, though no longer producing the exquisite subtlety of conscious gradation required for directly discriminating very adjacent touches. Physiologically expressed, the nervous messages are still sufficiently intact to travel along the well-worn subcortical paths, but have lost the perfection wanted for the delicate interactions in the cortex. Nor is the converse any less probable, namely, that the cortical paths may possess much power of adapting themselves eventually to chronic perversions of the nervous messages, whereas the sub-cortical paths—once failing—are scarcely able to recuperate.

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[1] This improvement refers to the variable error; the constant one depends upon other factors.

[2] It is hoped to complete the experiments and publish them in detail.

Thus, we have come upon three very different manners of localising; by direct comparison of sensations; by mediation of associated spatial images; and by mediation of associated pure 'thereness.' The skin can function in all three manners, while the joints are chiefly confined to the last named. It will be noted that *the three form a sequence in order of natural development by force of habit*; for the first can be conceived as growing into the second; the second into the third. We have to renounce the popular doctrine (so vigorously championed by Förster), that the movement-sensations from the joints are primitive and the spatial sense of the skin is merely a derivative therefrom: for the former seem to be in the less primitive stage of the two.

Still, there is no evidence for the contrary doctrine of Aubert and Kammler, that all spatial indications are wholly derived from those given by the skin; more probably all these functions have developed side by side, but the skin has been less hasty to part with earlier modes of function. Nor is there any reason, so far, to accept their assumption that even the most primitive mode of function now observable is ultimately innate; I find more evidence that even this is only a genetic product like the others, and that the psychologist has little more chance of observing directly the real original localising function than the geologist has of looking at the original condition of the earth's crust. In the ancient dispute of Aubert and Kammler with Leyden, the former had the merit of announcing the important fact, that discrimination of two points differs from groping for a spot; but their distinction, admittedly of philosophical rather than psychological character[1], was inadequately defined, involved unwarrantable assumptions and, above all, remained totally unproved; so that its rejection was justifiable enough. Reserving the difficult question of ultimate genesis for another occasion, the present practical inquiry has merely to note that spot-finding allows two additional factors to creep in: a preliminary step of simple localisation, and a mechanisation of the whole process by habit. The clearer meaning and more constant results of the method with the compasses, especially when applying the points simultaneously, have always secured for it the favour of the best authorities[2].

It may, perhaps, be remarked that throughout no allusion has been made to two old friends that generally enjoy the chief honour in tactile pathology, the 'active touch' and the 'muscular sense.'

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[1] See Aubert's *Physiologie der Netzhaut*, S. 8 and 9.

[2] See Wundt's *Phys. Psychologie*, 5th ed. Bd. II. S. 441 and 444.

The former, or power to recognise objects by feel, has often been of practical use—and has sometimes been the occasion of new psychological theories[1]. But there is good theoretical and practical reason for considering it merely a combination of two more elementary factors, the various spatial perceptions and the discrimination of pressure[2].

The more celebrated 'muscular sense' has not even the virtue of practical serviceability. Its name is bad, in that it attributes the function to the muscles, an opinion now generally discarded. Its denotation is no less faulty, for it throws together a motley group of disconnected activities; in its most moderate usage it confuses the sense of position and the perception of weight, two things both psychologically and physiologically independent of one another[3]; the former is based, as we have seen, upon the articular and segmental indications; while the origin of the latter is still rather obscure.

§ 10. *Inconsistency with the results of Förster.* This able investigator's conclusion, that the localising power is always completely dependent upon the efficiency of the movement-sensations, has thus had to be decisively contradicted. The question still remains as to whether the mistake has occurred in his work or in this. The latter alternative not being agreeable to the present writer, a brief attempt will be made to suggest the conceivability of the former.

Förster lays great stress upon precise *quantitative* tests and has—for the first time in the history of the question—determined his patients' localising powers in this, the only adequate manner; but as regards the other senses compared, those of contact and movement, he has unfortunately remained content with methods of the usual crudeness and unreliability; yet these two senses are, if anything, in still greater need of exact treatment[4]. This deficiency becomes vital when we consider that this argument is based throughout, not upon the broad difference between soundness and injury, but upon fine distinctions between injuries of believed greater and less magnitude.

Further, in most of his cases the degradation of localising power has not been ascertained by comparing together the two sides of the same person, but by reference to supposed 'normal' values. Such reasoning, though legitimate enough in principle, requires very cautious application. Now, Förster's data for this purpose seem much too scanty to justify more than mere suggestion; for some parts of the body they do not at all coincide with my own experience[1].

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[1] For example, see Wernicke, *Arbeiten a. d. psychiat. Klinik in Breslau*, 1895.

[2] See Hoffmann, *Arch. f. klin. Medicin*, Bde. XXXV. and XXXVI.

[3] The physiological independence of these two faculties was clearly demonstrated by Leyden, see *Virchow's Archiv*, Bd. XLVII.; again by Hoffmann, *loc. cit.*

[4] On the impossibility of properly testing the movement-sensations without proper apparatus see Frenkel, *Neur. Centralblatt*, 1897, S. 688 and 734.

Added to this, his whole system of comparative tests is based upon the assumption that "under normal circumstances the intensity of the stimulus makes no difference; a very fine touch is just as well localised as is a strong pressure[2]." He does not mention whether this assumption rests upon any exact evidence. My own work has happened to lead me to experiments indicating, on the contrary, that the strength of pressure is a very influential factor; and only thus can I explain the curious fact that his patients almost always localise actually better on the anaesthetic side than on the healthy one!

Finally, even if localisation really did prove to be wholly correlated with the so-called movement-sensations, as Förster believes, it would not thereby have shown any special connexion with movement but only with the articular excitations in general.

§ 11. *Principal Conclusions.*

I. Tactile localisation cannot be treated scientifically until it has been analysed into its several component factors. Any of these may or may not be influential, according to the localising procedure and to the part of the body under test.

II. The sensory excitations upon which localisation depends are of two distinct kinds, in this paper termed 'articular' and 'segmental' respectively. Among other differences, they differ notably as to their paths of transmission up the spinal cord and as to their sensory areas in the brain; the articular paths do not decussate below the medulla, and —both in spine and brain—are gathered into very compact groups for each joint.

III. These excitations may be effective in three independent ways: by directly producing sensations endowed with distinguishable gradations; by evoking mental images (visual or tactual) of the limb in question; or by transmitting merely physiological indications, which have to undergo an intricate combinative and assimilative process before engendering any spatial percept. Though these three ways now present some remarkable contrasts, both normal and pathological, they appear to mean only different stages of mechanisation by habit.

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[1] For instance, as 'normal' error of localisation on the lower part of the leg, Förster appears to rely upon three values obtained by himself and amounting to 0·7, 0·8, and 1·8 respectively; I have used just the same procedure very frequently and have found the average no less than twice as large.

[2] *Monatschr. f. Psychiatr. u. Neurol.* Bd. IX. S. 33.

IV. The so-called 'movement-sensations' have no present direct influence upon our power of localising; but debasement of the former is to a certain definable extent accompanied by that of the latter, because both are intimately connected with the articular excitations. (This conclusion, however, leaves untouched the question of genesis *originally*.)

S.'S SENSIBILITIES (WHEN UNTIRED): PRINCIPAL RESULTS OF THE TESTS.

Segment	Test	S.		Normal[1]
		Right	Left	
Thighs...	Localisation, simple	4·5 cm.	4·1 cm.	3·5 cm.
	,, looking......	5·5 ,,	3·2 ,,	2 ,,
	,, groping......	6·2 ,,	3·8 ,,	1·6 ,,
	Hair test	60 gm.	24 gm.	6·4 gm.
	Compasses....................	Over 8 cm.	Over 8 cm.	6·7 cm. (5 ,,)
	Movement, hip..............	0·7°	1·0°	0·8°
Calves...	Localisation, simple	4·6 cm.	10 cm.	3·5 cm.
	,, looking......	3·6 ,,	3·1 ,,	2 ,,
	,, groping......	4·2 ,,	3·6 ,,	1·5 ,,
	Hair test	14·4 gm.	7·2 gm.	5·8 gm.
	Compasses....................	3·5 cm.	3·5 cm.	4 cm. (4 ,,)
	Movement, knee	0·8°	20°	0·8°
Feet......	Localisation, simple	3·1 cm.	9·7 cm.	3 cm.
	,, looking......	1·9 ,,	1·9 ,,	1·5 ,,
	,, groping......	1·7 ,,	1·7 ,,	1·1 ,,
	Hair test	12 gm.	12 gm.	4 gm.
	Compasses....................	2·2 cm.	2·3 cm.	4 cm. (2·5 ,,)
	Movement, ankle...........	1·2°	Over 40°	1·5°
Toes......	Localisation, simple	3·5 cm.	12 cm.	3 cm.
	,, looking......	1·4 ,,	1·7 ,,	0·7 ,,
	,, groping......	1·3 ,,	1·6 ,,	0·4 ,,
	Hair test	11 gm.	6 gm.	3 gm.

[1] The mean deviation is about 10°/₀ either way.

V. The sense of contact correlates with the power of localisation in a variable manner. If a dulness to contact be temporary, the sensational gradations may be debased, but the images and the physiological indications spared; if the dulness be chronic, the latter will more probably be affected, while the former escape unhurt.

VI. The above analysis appears to explain readily the hitherto unaccountable morbid state known as Allocheiria. It also solves the long-standing quarrel between the 'Sense of Space' (*Raumsinn*) and the 'Sense of Locality' (*Ortsinn*).

Values gained from S.

Those for localisation represent the median variable errors; on the feet and toes the tests were confined to the upper surfaces; on the calves they were applied to the area of the fourth lumbar nerve; on the thighs to that of the second lumbar nerve.

Those for the hair test represent the maximum weight that the hair which showed his threshold was strong enough to overcome; the weight was placed in one scale-pan and the hair was pressed down on the other. The diameter of the contact surface at the end of the hair was 0·8 mm. The stimulation was applied midway between the hairs on the patient's skin.

Those for movement represent the extent at which S. could just decide whether it was upwards or downwards (about eight times rightly out of ten). Rate, 1° per second.

The inaccuracy to be feared from mere chance fluctuations is not easy to calculate definitely, especially in the case of the hair and the movement tests. As before mentioned, the whole observational basis amounts to some 3800 single experiments; roughly speaking, any of the above 44 mean values finally attained may be expected to deviate by chance from the true mean (that which would be given by infinite experiments) to an extent of about 5 to 8°/₀.

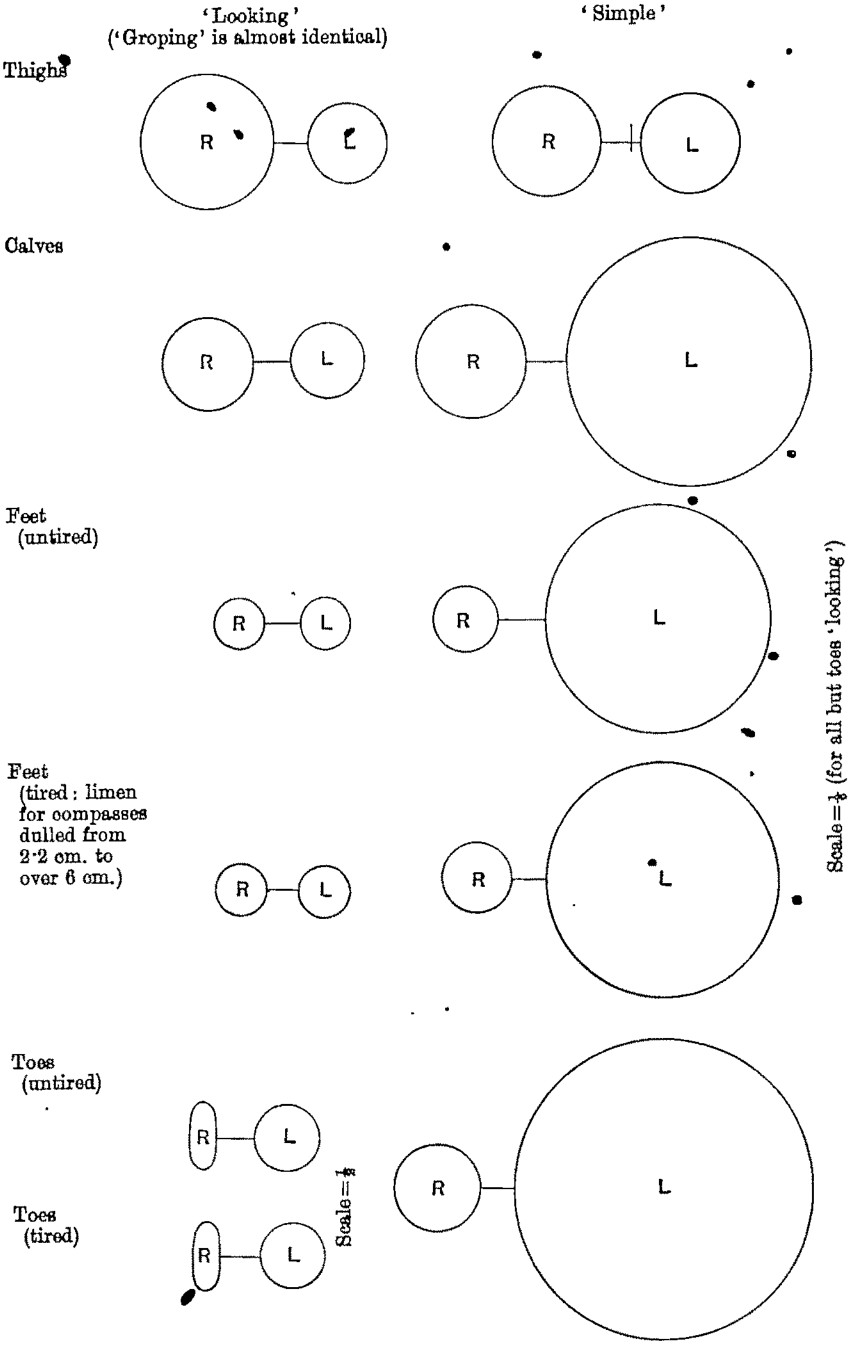
Normal values.

Those for movement were collected from the experiments of Leyden, *Virchow's Archiv*, Bd. XLVII.; of Goldscheider, *Archiv für Physiologie*, 1889; of Mülhens, *Diss.*, Bonn, 1897; of Frenkel, *Neurolog. Centralblatt*, 1897, S. 688 and 734; and of Pillsbury, *Amer. Journ. Psych.*, Vol. XII. All these agree remarkably well with one another and with my own results.

Those for localisation (all three procedures) and for the hair tests are derived from experiments of my own.

Those for the compasses are the classical ones of Weber; but my own experiences are shown also (in brackets), as they do not altogether coincide with those of Weber. For instance, Weber gave the same threshold for the foot as for the calf, whereas I have invariably found that for the former to be much less.

VARYING ACCURACY OF S.'S LOCALISATION, ACCORDING TO REGION AND PROCEDURE.


'Looking'
('Groping' is almost identical)
'Simple'
Thighs
Calves
Feet
(untired)
Feet
(tired: limen for compasses dulled from 2·2 cm. to over 6 cm.)
Toes
(untired)
Toes
(tired)
R
L
Scale=$\frac{1}{12}$
Scale=$\frac{1}{6}$ (for all but toes 'looking')

Any semi-diameter gives the median variable error (about equal in every direction, except as regards the toes; for these, the front is represented by the top of the paper). Thus, each outline encloses about the best *half* of the localisations.

PROCEEDINGS OF THE PSYCHOLOGICAL SOCIETY.

June 25, 1904. Theories of Consonance and Dissonance, by CHARLES S. MYERS.

The theories which have been advanced to explain the differences of consonance and dissonance, may be ranged under three heads. The first rests on the actual experience rising from the presentation-complex, the second is founded on the relative rate, form or coincidence of the sound-vibrations, the third takes into account both physical and psychical considerations.

To the former belongs the fusion-theory of Stumpf, an unconscious revival of the ancient Greek conception, which makes the consonance or dissonance of an interval dependent on the extent to which the component tones blend together as a unity in sensation. The grade of this fusion is estimated by the difficulty of analysis of the interval. Corollaries, consequent on this theory, hold that (*a*) the distinction between consonance and dissonance is one not of kind, but only of degree, and that (*b*) the replacement of one of the tones of an interval by its higher or lower octave (or double, treble, etc. octave) makes no change in its degree of consonance. Stumpf's theory, however, is little more than an explanation of the unknown by the unknown, bringing us no nearer the nature of 'consonance' and 'dissonance' than would a consideration of the literal meaning of these words. Moreover, fusion is naturally not the sole test of identity, nor the sole determinant of analysis. For Stumpf shows that intervals presenting the same degree of fusion may yet be dissimilar, owing to the different sizes of those intervals or the revival of various memories. And he allows that equally fusing intervals may differ in their ease of analysis, according to the relative distance, duration and intensity of the contained tones. Finally to apply his fusion-principle to simple melody, Stumpf supposes that the selection of (*consecutively*) consonant tones in primitive music depended on the chance *simultaneous* hearing of such tones in song or on instruments,—a conjecture not only wholly unnecessary but directly opposed to our knowledge of the conditions of at least much of modern uncivilized music.

To the second group belong theories which make consonance and dissonance dependent on the unconscious appreciation of the ratio of the component vibration-rates or on the periodic coincidence of sound-waves. These deserve merely passing consideration.

To the third group belong the two definitions of Helmholtz:—(A) Consonance is a continuous, dissonance is an intermittent tone-sensation. Objection may be taken (i) to the phraseology by which the theory is expressed, (ii) to the exclusive dependence of dissonance on the presence and number of beats, and (iii) to the applicability of the theory solely to simultaneously and not to successively sounding tones of an interval. The second objection suggests further experiment and general considerations. The third was foreseen by Helmholtz, who gave a supplementary definition (B):—The consonance or dissonance of an interval depends on the degree of coincidence or of dissimilarity of the overtones of the component tones. If this definition held, consonant intervals should not be recognized when their overtones are abolished, but the experimental evidence on this point is conflicting; also two consecutively given tones would be consonant or dissonant only by association of the former of the tones with the latter in memory.

A theory which is based on a hypothesis suggested by Mach, rejected by Helmholtz and later enunciated more satisfactorily by Ebbinghaus, also deserves consideration. This hypothesis demands that a given tone of n vibrations per second should affect not only its appropriately attuned fibre of the basilar membrane but also those fibres attuned to tones of $\frac{n}{2}, \frac{n}{3}, \frac{n}{4}\ldots$ vibrations per second, causing them to vibrate from 1, 2, 3... nodal points respectively. According to this theory, two tones would be more consonant, the greater the number of basilar fibres stimulated in common, simultaneously or successively.

Lastly, I invite criticism of the following explanation of the most perfect consonances. We know that pure tones, resulting from pendular vibrations, can have no existence in nature. Consequently, whenever we judge that we hear a pure tone, n, we do so through a natural habit of thus interpreting the complex pattern of stimulus received at the cochlea: complex, because of the ever present overtones which are simultaneously stimulating their appropriate end-organs. Let us assume that the overtones contained in by far the majority of tones which affect the cochlea, form harmonic series; so that when a tone of pitch n is heard, the most strongly stimulated end-organ is, of course, that which is attuned to respond to the pure tone n, and the end-organ which is simultaneously stimulated next in order of strength and frequency is that which would be stimulated by the pure tone $2n$, the next in strength and frequency is the end-organ which would be stimulated by the pure tone $3n$, and so on. The result of such frequent experience throughout life is that when any tone of pitch x is given, there is no conceivable tone more closely associated and identifiable with it than that which will stimulate the end-organ attuned to the pure tone $2x$. That is, the octave (1 : 2) is the most perfect consonance. Similarly the tones x and $3x$, or the tones $2x$ and $3x$ are the next most closely allied pairs. Hence the twelfth (1 : 3) or the fifth (2 : 3) yields the most perfect consonance after the octave. Similar constant association of other overtones (3 : 4, 4 : 5, 5 : 6) may help to explain the less perfect consonance of fourths and thirds, which is however greatly influenced by the beating dissonant overtones that may accompany these intervals.

Visual Acuity in different Races of Man, by W. H. R. RIVERS.

A table was exhibited giving the chief results of ethnographical investigations of visual acuity. The table shows that racial differences in acuity are very slight when marked errors of refraction are not present.

Attention was drawn to various factors which should influence the choice of a test of visual acuity to be used for comparative purposes.

August 20, 1904. Neuro-Muscular Force: A Psychical Element in Muscular Force, by N. VASCHIDE.

A large number of dynamographic and ergographic experiments have convinced me that muscular force has a far more complex nature than is generally supposed, and that to our usual ideas of purely muscular force must be added a conception which, for lack of a better term, I propose to call 'neuro-muscular force' or 'subconscious muscular excitability.' At the onset of symptoms of insanity, typically in early cases of melancholia or of *folie circulaire*, muscular force differs widely in degree from that evinced by the same individuals in apparently normal health. A few further examples will make my meaning clear.

A. The mean of a number of measurements on the muscular force of a woman, aged 30, was found to be 27 kgs. She was a normal subject, but in the course of her life she had experienced several unusually violent shocks, considerable worry and unexpected emotions. One evening, under the impression that burglars were entering her house, she seized in self-defence the dynamometer which lay on her table and had been used each day to test her muscular force. The investigator was astonished to find a record of 53 kgs., an amount which in her natural state the subject could never attain even with her two hands.

B. Hysterical, somnambulant and epileptic subjects are capable of furnishing a considerable amount of subconscious muscular work in their crises, this neuro-muscular force reaching at a given moment an astonishingly high value.

C. In observations on the dying, I have chanced to notice an incredible amount of muscular force produced in the death-agony. I may cite the case of an aged and emaciated woman whose record on the dynamometer was barely 10 kgs., but who on her death-bed reached a pressure of 50 kgs.,—an amount producible only by a very robust person in normal condition.

All the motor elements of the organism are influenced by this neuro-muscular or subconscious muscular force, which manifests itself most clearly when the life of the subject is in danger. It is commonly the expression of a protective instinct, thanks to which those whose lives are in jeopardy can employ great force. By reason of it somnambulists and those suffering from delusions are able to acquire extraordinary power.

I intend to pursue the subject in greater detail hereafter.

The Bearing of Modern Experimental Work on the Problem of the Unity of the Mind, by W. McDOUGALL.

After describing the principal bundles of nerve-fibres connecting different parts of the cortex of the cerebrum, von Hartmann writes, "It is only the abundance and excellence of these paths which make possible such a facile psychical communication of all the ganglionic cells of the anterior brain with one another that their more vivid perceptions flow together into a single consciousness," and in the same work (*Die Philosophie des Unbewussten*) we read, "Only because the one part of my brain has a direct communication with the other is the consciousness of the two parts unified; and could we unite the brains of two human beings by a path of communication equivalent to the cerebral fibres, both would no longer have *two* but *one* consciousness"; and again, "The capability of conduction it is then, in fact, which physically conditions the unity of consciousness and with which this is proportional."

In these and in other passages of similar import von Hartmann clearly asserts and boldly carries to its logical conclusions the principle that the unity of the mind is a function of the unity of the body and is determined by the abundance of nervous connexions between all the parts of the brain. This principle is accepted by most of the physiologists and by many of the leading psychologists of the present time. It would seem in fact to be an assumption necessarily made by all those who accept the principle of psycho-physical parallelism in any thorough-going fashion. The validity of the assumption may be profitably discussed at the present time because recent researches have brought to light two classes of facts that bear upon the question.

The one class consists of the strange phenomena of 'psychological disgregation' described and fully attested by Dr Pierre Janet and several other reliable observers. In these cases we seem to see the mental processes of a single bodily individual divided to form two streams of presentation, feeling and volition, that flow on *simultaneously* but quite independently of one another. In some cases, usually spoken of as cases of split personality, the two streams seem to constitute two new personalities or psychical individuals of equal complexity. In other cases, *e.g.* cases of partial hysterical anaesthesia and paralysis, the one stream seems to be but a small fragment of the normal personality which remains mutilated or diminished in complexity in a corresponding degree.

If we accept the principle enunciated above, these cases may be explained by assuming that the nervous elements of which the brain is made up, normally a single functional group of great complexity, fall into two such groups, the paths of conduction, that normally serve to connect the members of the one group with those of the other, ceasing to function. This is the explanation usually adopted, and, since it is very difficult to suggest any other, these cases seem to afford support to the principle that the unity of the mind is wholly conditioned by the anatomical continuity and functional unity of the nervous system.

But evidence of another kind seems to show that the principle is an untenable assumption. It consists in all the facts that tend to prove a more or less strict localisation of functions in the brain. Of such facts those afford the clearest guidance which prove that any two 'corresponding' retinal elements are not directly connected with the same nervous elements in the visual area of the cerebral cortex, and that the nervous processes initiated in them on stimulation do not in any way fuse to a common resultant, while the psychical effects of the two separate nervous processes do thus fuse. These facts may be grouped as follows:—

(1) The influence of voluntary attention in determining the predominance in consciousness of one or other of two dissimilar fields presented to the two eyes. (This was accepted by Helmholtz as proof of the separateness of the physiological processes.)

(2) The formation of new correspondences between retinal points in cases of squint.

(3) Cases of partial blindness of one eye only resulting from small lesions of the visual cortex.

(4) Prof. Sherrington's observations published in the *British Journal of Psychology*, Vol. I. No. 1.

(5) Certain observations of after-images which show that, when corresponding retinal areas are simultaneously stimulated, two independent after-images remain which may either fuse to a common resultant or appear separately and alternately.

If the principle of specific energies of sensory nerves (taken in the widest sense) may be regarded as well-established, other sensory processes, especially the fusion of simple auditory tones to form clangs and compound tones, must be regarded as affording evidence of the same kind, because they must be regarded as instances of purely psychical fusion.

The consideration of more complex processes compels us to the same conclusion; *e.g.* the visual perception of the position, form and size of any object somehow embodies besides visual sensations, a varied group of sensations of which the kinaesthetic are probably the most important, and these are psychical elements determined by nervous processes taking place in anatomical elements in different parts of the brain cortex; or again, in reading aloud a sentence (with understanding of its import) the nervous processes underlying the complex but unitary psychical process, which we call the understanding of the sentence, take place in anatomical elements that are widely scattered through several of the great sensory areas of the cortex as well as in the so-called 'association' areas. Modern knowledge of the structure of the brain forbids us to assume that all these scattered nerve-elements, or groups of elements, propagate their specific forms of activity to some one central organ or nerve-element, as was formerly supposed to be the case. Nor can we assume that the process in each one modifies, and is reciprocally modified by, the process in each of the others so as to determine in all of them alike (as von Hartmann supposes) a complex form of vibration, the resultant of the several simpler forms proper to the several nervous elements; for we have seen that this does not occur even in the case of nerve-elements that are parts of one functional area of the cortex (namely the visual area).

Nothing remains but to accept the principle of purely psychical fusion of psychical elements without corresponding fusion of the nervous processes underlying them, or, as Wundt calls it, the principle of 'creative resultants.' If this conclusion is well-founded we must regard the principle of psycho-physical parallelism as a premature attempt to render intelligible what can only be rendered intelligible by future research and thought, and we shall seek to build up our psychology on a frankly and completely dualistic basis, a truly physiological psychology based on the principle of interaction of soul and body. To many this will seem to be the view of a reactionary obscurantist, but it is the logical outcome of conclusions that were accepted by Helmholtz and by Lotze and are now maintained by Wundt, conclusions for which the progress of experimental research provides every year a firmer basis.

The Illusion of Compared Horizontal and Vertical Lines with Momentary and Prolonged Exposure, by W. H. R. RIVERS and G. DAWES HICKS.

The authors presented a preliminary account of an investigation which consisted in studying by means of the method of right and wrong cases the influence of the duration of exposure on this illusion in three individuals. Observations were made both with constant horizontal, the length of the vertical varying, and with constant vertical, the length of the horizontal varying.

The illusion was found to be of almost exactly the same amount when the lines were exposed momentarily as in prolonged exposure.

The three observers agreed in forming a larger number of distinct judgments with momentary exposure. In the case of two observers the indefinite judgments ('same or longer,' 'no difference,' 'same or shorter') were less frequent with the momentary exposure. The illusion was more obvious when the lines were only exposed momentarily than in prolonged exposure. This agrees with the results of introspection. In the momentary exposures the differences in the length of the lines often seemed very considerable, and when there was uncertainty this seemed due to imperfection of fixation. With prolonged exposures, on the other hand, the formation of a judgment was often very difficult, but the uncertainty in this case was due to hesitation, and often to oscillation, of judgment.

Each observer noted a tendency to compare one line, horizontal or vertical, not with the line simultaneously presented, but with the corresponding line seen in a previous exposure.

VOLUME I DECEMBER, 1905 PART 4

OBSERVATIONS ON THE SENSES OF THE TODAS.

BY W. H. R. RIVERS.

[*The work recorded in this paper was carried out with the aid of the Gunning Fund of the Royal Society for the years* 1901–2.]

Introduction.

Visual acuity. Ocular defects.

Colour-vision: nomenclature, discrimination, colour-blindness.

Visual illusions: the illusion of compared horizontal and vertical lines; the Müller-Lyer illusion.

Tactile discrimination; Aristotle's illusion and its modifications. Sensibility to pain.

Olfactory acuity. Taste names. Hearing.

Conclusions. The part played by inference in the determination of a threshold. No definite racial differences in the acuity of the senses. Influence of age on acuity. Relation between colour-nomenclature and colour-sense. Frequency of colour-blindness in the Todas. Some illusions experienced more strongly by the savage than by the civilised subject, and others less strongly. Variability in psychological tests and the factors by which the measures of variability are influenced. Correlation between the simple mental operations tested and general intellectual development.

THE Todas of the Nilgiri Hills in Southern India are a small community of about 800 individuals. Physically they are a fine and handsome people, differing considerably in bearing and general appearance from any other tribe or caste of Southern India. They lead a simple existence, their lives being devoted chiefly to the care of their buffaloes, and during a long period of isolation on the Nilgiris they have had no keen struggle for existence. At one time they seem to have lived largely on roots and berries, but this source of food-supply has for a long time been replaced by grain and rice procured from other tribes; and even when they had to seek out their food it was probably easy to find. They have had neither the arduous search for food nor the necessity of being continually on the guard against enemies, human and animal, which have done so much to develope the powers of observation possessed by such races as the Australians and American Indians.

During the last fifty years the Todas have been undergoing a certain amount of degeneration, physical and moral, but this has been much more marked in some divisions of the community than in others, and many of those whom I tested had been little influenced.

The Todas may be taken as an example of a race in which there has been no great incentive to extraordinary development of the sensory powers, nor till recently have there been any causes at work to produce pathological changes in the sensory organs.

My observations were made by methods as similar as possible to those used by the Cambridge Expedition to Torres Straits[1]. In some cases modifications were necessary, but my aim was to obtain material which would be exactly comparable with that obtained in Torres Straits. The general method of work was the same. The people came to me in groups of three or four persons to pass the morning in being tested, and while one individual was being examined, the others were usually employed in conversation or in looking at portraits of themselves, an occupation of which they were never tired. Ample opportunity was thus given for recovery from any fatigue or *ennui* due to one test before going on to another. The people usually came to the place where I was living, and I moved about to various centres on the Hills, and in this way was able to examine all sections of the community. I also visited most of the Toda villages occupied at the time of my visit, and most of my observations on women and children were made during these visits.

In one important respect my work with the Todas differed from that in Torres Straits. The great majority of the inhabitants of the Straits knew some 'pidgin' or jargon English and no interpreter was necessary. With the Todas I had to use an interpreter, and though after a time I acquired sufficient knowledge of the vocabulary connected with each test to enable me to work alone I had to call upon my interpreter for help whenever I took up a new test. The fact, however, that I was never able to communicate fluently with the natives, even in so defective a medium as the 'pidgin' English of Torres Straits, make my observations on the Todas in some respects less satisfactory than those on the

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[1] For a general account of the aims and methods, cf. *Reports* of this Expedition, Cambridge, 1901, Vol. II. Introduction, pp. 1—7.

Papuans. But this disadvantage is partly counterbalanced by the greater intelligence of the Todas, and I do not believe that the linguistic difficulties have led to any important defects in the work.

I had no difficulty in persuading the Todas to make the observations I required[1]. Each person who came to me was paid a given sum as remuneration for his time and trouble, but anything like payment by results was carefully avoided and no artificial inducements were offered to promote competition between the natives. In cases where the people could themselves compare their results, as in testing visual acuity, they were interested in seeing how well they had done, but there did not appear to be any keen rivalry among them. Nevertheless the care and attention devoted to the observations were admirable; occasionally a native would be careless and inattentive, but such cases were even rarer among the Todas than in Torres Straits. As we shall see later, the smallness of the mean variations in the case of the quantitative observations shows that these observations must have been made honestly and carefully.

My experience among the Todas corresponds with that which I have gained elsewhere. The advent of a European into a community of low culture which he wishes to study awakens so much interest among the members of the community that they give their full attention without any artificial inducements. The state of affairs is very different when members of a community of low culture are brought to Europe. Here, the would-be investigation is merely one among a large number of novel features of the environment. The attempts to investigate excite little or no interest, and except in a few very simple matters it is impossible to obtain that amount of attention and care which gives the observations any serious value.

Visual acuity.

The visual acuity of the Todas was tested by means of the modification of the E test devised by Cohn, in which a letter E can be exposed in any desired position through a circular hole in a card. The subject of the test has to place a letter E which he holds in his hands in the same position as one shown to him. Instead of the small cardboard E provided in Cohn's test, I used a larger letter E pasted on a board. Cohn's method is very simple and convenient and it entirely removes the danger accompanying the older tests, that the letters may be learnt by heart during the process of testing.

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[1] In my work with the Todas I was undoubtedly very much helped by their knowledge of the support given to me by the authorities of the Madras Government and of the Nilgiri District. I should like to take this opportunity of expressing my thanks for their help, and especially for that of Mr Edgar Thurston, to whose kind assistance I owe very much.

In general the procedure was the same as that previously adopted in Torres Straits; the observations were made in the open air, both eyes were used, and the distance at which a native made two mistakes in ten exposures was taken as his limit of vision. In one respect the procedure differed; with the older form of the test it was most convenient to begin with the observer beyond his far limit of vision and to bring him up towards the test-types till he could decipher the letters. With Cohn's form of the test, I first showed the E in various positions at a short distance, and as soon as I had satisfied myself that the native understood the method of testing, I gradually increased the distance till I reached a point at which the positions of the letter could no longer be recognized.

The great majority of the Todas quickly learnt the method, and it was rarely that I had any difficulty in obtaining satisfactory results with the men. I had more trouble with the women, and failed entirely to make some of the older women understand the test, and in other cases their behaviour was so erratic that I did not feel confident in the trustworthiness of their observations.

I have considered the various sources of error elsewhere[1]. Further experience has confirmed me in my opinion that the changes of illumination which occur in the open air do not materially affect visual acuity. Changes in the illumination of a room are undoubtedly of influence, but in the higher illumination of the open air differences have much less influence. Both in India and Great Britain I have often tested the same individuals in the open air on bright and dull days and found no difference in the limit of vision. If the illumination is very bright English observers may be troubled by the glare, and better observations are obtained by moving into a shaded place, but with this exception I do not believe that such changes of illumination as occur in the open air during the greater part of the day, unless extreme, have any appreciable influence on the results given by the E test[2].

I tested 84 Toda men and boys. The average distance at which they could distinguish the position of the letter E was 13·3 metres. Since the letter used was one which is supposed to be seen by the normal eye at 6 metres, their vision would according to the system in ordinary use be put down as 13·3/6 or 2·22 times the so-called normal. Fourteen women and girls distinguished the positions of the letter at an average distance of 12·5 metres, so that their visual acuity would be expressed by 2·08, slightly less than that of the males. The average acuity of the whole group of 98 individuals is 2·20. The individuals tested were of all ages, and may be taken as a representative sample of the whole community.

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[1] See *Reports*, pp. 20—28.

[2] By this it is not meant that the recognition of distant objects in ordinary life is not influenced by illumination. This is a much more complex process than that involved in the experimental determination of visual acuity.

Giving the results on the same plan as that adopted in the Torres Straits reports, 3·1 °/₀ have visual acuity expressed by 1 or less; 34·7 °/₀ have an acuity between 1 and 2; 60·2 °/₀ between 2 and 3, and 2·0 °/₀ between 3 and 4. In no case was the visual acuity more than four times the so-called normal, the greatest distance at which the E was recognized being 22 metres.

The influence of age on visual acuity is best shown by comparing those above 35 years old with those below. The average distance at which the E could be recognized by the former was 11·0 metres; by the latter 14·6 metres.

If the figures be compared with those of the Table given on p. 25 of the Torres Straits *Reports*, it will be seen that there is little difference between the Todas and other races. The Todas have a somewhat higher average acuity than the inhabitants of Torres Straits and fewer subnormal cases, but this difference is due partly to the fact that an undue proportion of elderly men were tested in Murray Island, partly to the greater frequency of pathological conditions of the cornea and lens in that island. If those less than 40 years of age in the two communities are compared, the Papuans have a slightly better acuity than the Todas[1].

The observations on the Todas confirm the conclusion arrived at from work in Torres Straits and elsewhere that when errors of refraction and other pathological conditions are excluded, there is no great difference in visual acuity between savage or barbarous and civilised races, though the balance of superiority may be slightly on the side of the former.

The Todas gave no evidence of any special development of powers of visual observation, so that the ordinary observer would probably have judged their acuteness of sight to be much below that of the Torres Straits islander.

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[1] I propose to deal more fully with the comparative evidence now available in a later paper.

I discovered no case in which subnormal vision among the Todas could be ascribed to myopia or other errors of refraction. Only seven men failed to recognize the positions of the letter at less than 10 metres, and these were all elderly men whose low visual acuity was due to the changes which occur with advancing age. If any Todas had refractive errors, they must have been of such a nature as not to interfere with the recognition of a letter of the No. 6 size at 10 metres.

Cataract is fairly common among the Todas; two people, a man and a woman, had been operated on for this condition, and a third operation took place during my visit. Some of the seven men just mentioned probably had incipient changes in the lens, and another man, the oldest living Toda, who was over 80 years of age, had defective vision, probably owing to lens changes, but I was not able to determine his visual acuity exactly. One man was totally blind, although no defect could be seen in his cornea or lens.

Some pigmentation and injection of the conjunctiva was very common. In some people there was the same kind of definite ring of pigment surrounding the cornea which I have described in Torres Straits. Several had definite pinguicula, and others thickening in the usual position of pterygium, but I saw no definite case of the latter condition among the Todas. True arcus senilis was observed in several cases, and the superficial haziness resembling arcus in others. In slighter cases of this superficial haziness, the appearance was that of a bluish ring at the outer edge of the cornea, such as was observed in Torres Straits.

Colour-vision.

For the investigation of the colour-vision of the Todas I used the same methods as in Torres Straits[1] and Egypt[2].

I first tested with Holmgren's wools, then obtained the names of colours in various ways, and in some cases made observations with Lovibond's tintometer or tested the colour-vision of the peripheral retina. I will begin with an account of the words used in naming colours.

Colour nomenclature. This was found to have the same general characteristics which I have found elsewhere. There was a perfectly definite word for red, viz. *pògh*[3], blood, which was used by everyone with normal colour-vision for reds of all kinds. Orange was sometimes also called *pògh* but was more often called *mun*, earth, or *kemun*, red earth. Yellow was called *ársena* with great unanimity. This is probably not a true Toda word but borrowed from Canarese through intercourse with the Badagas. Green received many names, including *ers*, leaf; *ŏmadi*, moss; *pachai*, the Tamil word for green; *nîl*, blue and *kâg* dark or black. Blue and indigo were called *nîl*[1] most frequently but also *kâg* or *kârthiti*, black. The nomenclature for violet was very indefinite; some could not give it a name; others compared it to some natural object, and others called it *nîl* or *kâri*, charcoal. The nomenclature for brown was equally indefinite. Papers of different shades of brown were named by the majority after some variety of earth or were compared to sticks, leaves, skin, soot, beeswax, etc., and were occasionally called black, white, or yellow. It was quite clear that there was no generic word for brown. White was unanimously called *pelthiti* ("it is white") and black was called *kârthiti* ("it is black") or *kâg* or *kâri*. Different greys were usually called *pelthiti* if light or *kârthiti* if dark; a few people called light grey *pûthi*, ashes, a form of the common South Indian word for this colour.

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[1] *Loc. cit.*

[2] *Journ. Anthrop. Inst.* 1901, Vol. XXXI. p. 239.

[3] The sign ò is used for the sound of *aw* in the word law.

In 1880, Magnus[2] on the authority of a missionary, described the Todas and other Nilgiri tribes as having a definite colour word only for red. If the borrowed words, *ársena* and *nîl*, be excluded, it is still correct to say that red is the only colour for which the Todas have one term used with any degree of unanimity, but they are nevertheless able to name other colours by means of words of which the derivation shows that they are used suitably and that the colour has been discriminated. In its present state the colour-vocabulary of the Todas has reached very much the same degree of development as that of the Egyptian peasant or the native of Torres Straits. There are definite words for red and yellow, less definite nomenclature for green and blue; blue and black are often given the same name, and there are no definite terms for brown or violet.

The Todas also resembled the natives of Egypt and Torres Straits in their liking for comparing the colours of papers or wools with natural objects, with flowers and plants, with various kinds of earth, and often also with articles of clothing. The tendency to compare with natural objects was very marked among those suffering from colour-blindness, and it reached its highest degree of development in a very old man, who compared different colours to different conditions of the sky. Blue was said to be like the sky when the sun is hiding behind the clouds; indigo was like the rising clouds of the monsoon; white was like shining after rain; one grey was like a cumulus when the sun is shining brightly; another grey was said to be "the sun is setting and it is getting dark," and a brown was like the dawn when daylight is coming.

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[1] *Nîl* or *nîlâ*, derived from Sanscrit, is used for blue by all the Dravidian races of South India.

[2] *Untersuchungen über den Farbensinn der Naturvölker*, Jena, 1880, S. 7.

These comparisons were made by one of the oldest of living Todas and are therefore especially interesting as showing the close observation of nature which is found so often among primitive people, while at the same time they point to an almost artistic appreciation of light and colour effects. It is probable that these powers of observation and appreciation are to be numbered among those features which are disappearing under the altered conditions to which the Todas are exposed.

Colour discrimination. Five hundred and three individuals were tested with Holmgren's wools. Forty-three were found to be definitely colour-blind, and their observations will be considered later. The remainder showed exactly the same peculiarities which I have found in Torres Straits, Egypt, and elsewhere. Some natives matched the wools perfectly, but the great majority confused red with pink, green with blue, violet with blue, and faintly-coloured wools generally tended to be confused with one another. One intelligent man, who otherwise did well, matched blue with grey and brown wools, and two others confused blue and brown. The defects of this kind were less marked than in Egypt or among the Uralis and Sholagas whom I examined in India[1], but were very much the same as in Torres Straits. They all show a tendency to discriminate greens, blues and violets less definitely than reds and yellows, and show that the deficiencies in nomenclature for the former group of colours are accompanied by a certain degree of deficiency in their discrimination.

Colour thresholds. Quantitative observations were made with Lovibond's tintometer in exactly the same manner as in my Egyptian work[2]. The examination is one which demands much care and attention and this was given by the Todas, so that there were very few cases (except those of colour-blindness) in which I failed to obtain satisfactory determinations of the colour thresholds. Usually only one pause was necessary during the examination, but occasionally a further period of rest was given.

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[1] *Bull. Madras Government Museum*, Madras, 1903, Vol. v. p. 3.

[2] For a full description of this method with a sample case see *Journ. Anthrop. Inst.* 1901, Vol. XXXI. p. 239.

TABLE I.

Colour Thresholds of 47 Toda men.

	Red	Yellow	Blue
Average	32·1	29·2	53·3
Maximum ...	100	120	100
Median	30	20	50
Minimum ...	10	5	15
M.V.	18·1	15·6	20·7
σ.	24·3	21·3	23·4
C.V.	75·5	73·1	44·0

In Table I are given the average threshold for each colour of 47 Toda males, together with other data. The threshold for red is slightly higher than that for yellow, but both are considerably lower than the threshold for blue. The mean variations, standard deviations, and coefficients of variation are all very large, a fact which will be more fully considered later.

Observations were also made on seven women. Their average thresholds were: for red, 23·6 ($\sigma = 9·2$); yellow, 25·0 ($\sigma = 14·4$); and blue, 54·3 ($\sigma = 14·0$). The number tested is very small, but the averages for red and yellow are distinctly lower than for the men, while that for blue is about the same. The standard deviations of the women are very distinctly lower than those of the men.

The Todas have two distinct divisions which do not intermarry, and in one, the Târthârol, colour-blindness is rather more frequent than in the other, the Teivaliol, and corresponding to this, the members of the latter division have somewhat lower thresholds. The standard deviations of the Teivaliol are also distinctly lower than those of the Târthârol.

TABLE II.

	Red	Yellow	Blue
Târthârol (29)	34·7, σ. 27·7	31·9, σ. 23·3	56·4, σ. 24·7
Teivaliol (18)	28·1, σ. 16·6	24·7, σ. 16·7	48·3, σ. 20·3

In order to ascertain whether age had any influence on the threshold, the men were divided into two groups above and below 35, the numbers of each group being given in brackets.

The results are as follow:

TABLE III.

	Red	Yellow	Blue
Above 35 years (16)	39·1, σ. 31·7	30·9, σ. 19·8	48·4, σ. 23·3
Below 35 years (31)	28·5, σ. 18·3	28·2, σ. 21·9	55·8, σ. 23·1

The most striking difference is in the threshold for red, which is decidedly larger in the older group of people. The difference is, however, less than twice its probable error, which is 5·8.

The interest of these tables lies in their relation to colour-blindness. This condition is much less frequent among the women than among the men, and is slightly less frequent among the Teivaliol than among the Târthârol and corresponding to this, we find that the women and the Teivaliol have lower thresholds, *i.e.* better colour-vision, than the men and the Târthârol respectively.

TABLE IV.

Colour Thresholds. Comparative Results.

Race	No. examined	Red	M.V.	σ.	C.V.	Yellow	M.V.	σ.	C.V.	Blue	M.V.	σ.	C.V.
Todas......	47	32·1	18·1	24·3	75·5	29·2	15·6	21·3	73·1	53·3	20·7	23·4	44·0
Uralis and Sholagas	14	31·1	13·5	16·6	53·4	26·4	7·9	8·7	33·1	66·4	15·5	17·6	26·4
Egyptians	26	28·7	14·2	15·8	55·3	26·0	10·5	12·3	47·5	85·4	34·8	43·4	50·9
Murray Islanders	17	17·6	7·5	8·9	50·6	26·8	9·9	11·6	43·0	60·0	16·5	20·2	33·6
English ...	41	27·5	17·9	21·7	79·0	16·7	7·2	10·0	60·1	30·8	11·2	14·9	48·4

In Table IV I give the results which I have now obtained in five different communities. In this table the average thresholds, expressed in units of the instrument, are given in the columns headed by the names of the colours. The figures under the heading M.V. give the mean variations of the results of the individuals of each group from the average result of that group. Under σ are given the standard deviations[1] of the individual results from the average; under C.V. the coefficients of variation[2] as worked out from the formula $\frac{\sigma \times 100}{\text{Average}}$.

This table shows that the Todas have the highest thresholds for red and yellow obtained in any community which I have tested. The differences, however, between the Todas on the one hand and the Sholagas and Uralis and Egyptians on the other are very slight and within the limits of the probable error.

More important is the difference between the Todas and the Papuans of Torres Straits. There is no great difference between the thresholds for yellow of the two communities, but the thresholds for red differ greatly, the difference, 14·5, being considerably more than three times the probable error of the difference[3], which is 2·8.

This difference is probably connected with the fact that among the Papuans red-green blindness does not occur, while among the Todas it reaches the highest frequency recorded in any community. The difference seems to show that even in those Todas who were able to pass the tests for colour-blindness, there was some deficiency in the sensibility for red as compared with the Torres Straits islanders.

For blue the Todas have a very distinctly higher threshold than the English observers, though lower than the other savage or barbarous races which I have tested. The difference between the figures for the English and Toda observers is enormously greater than the probable error of the difference which is only 2·8. The Todas confirm the conclusion reached in my previous papers, that the defective nomenclature for blue which is so generally found among races of low culture is associated with a certain degree of defective sensibility for this colour.

I am doubtful how far it is justifiable to use the standard deviation and other figures derived from it in measurements such as those I have been considering. In most cases the number of individuals tested is too small to enable satisfactory curves to be plotted, but even with the number available it is evident that the curves are asymmetrical; thus, to take the Toda results for yellow as an example, the following are the numbers of individuals at each threshold: at 10 units, 3; at 15 units, 12; at 20 units, 10; at 30 units, 6; at 40 units, 4; at 50 units, 4; and at 60, 70, 80 and 120 units 1 each. Further, the variations in the threshold are enormous as compared with those obtained in the physical measurements to which the method of calculating the standard deviation is usually applied.

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[1] I.e. $\sqrt{\frac{\Sigma\partial^2}{n}}$, where $\Sigma\partial$ is the sum of the squares of the individual differences from the mean, ∂_1, ∂_2 ..., and n is the number of individuals.

[2] I am indebted to Dr C. S. Myers for much advice in using these measures, and to Mr H. N. Webber for his assistance in their calculation.

[3] I.e. $\sqrt{\Sigma_x^2+\Sigma_y^2}$ where Σ_x and Σ_y are the probable errors of the two figures to be compared.

Nevertheless the figures obtained probably approximate to the values which would be satisfactory theoretically, and further I have been encouraged to give them in the hope that they may be of value as data for some future student of a subject which awaits satisfactory treatment, viz. variability in psychical characters.

Using the figures empirically, inspection of the coefficients of variation brings out one or two suggestive points. One is that the two races which have the lowest coefficients for each of the three colours are the Murray Islanders and the Urali-Sholagas. These communities are undoubtedly the two lowest of the table in point of culture and general intelligence, but at the same time they are the two communities in which colour-blindness is absent or very rare.

That it is the former of these factors which is probably the most important is indicated by the great variability of the English observers. These, belonging to the most highly placed of the five races in culture and general intelligence, have the highest coefficient of variation for red, and are exceeded only by the Todas in the case of yellow and by the Egyptians in the case of blue. These figures seem to show that there is an increase of variability with increase of culture in this measurement of colour-sensibility. How far the large coefficient is due to greater variability physiologically, *i.e.* in the actual sensitiveness of the retino-cerebral apparatus and how far to greater variability in the method of making the observations, we have no certain means of judging. There is no doubt, however, that the condition measured by this coefficient is complex.

It is probable, however, that the high degree of variability in the group of English observers is partly due to differences in the methods of making the observations which leave a certain amount of scope for inference; a person may say that a glass is blue not because he sees blue but because he sees one field darker than the other, and as he does not recognize either red or yellow, he infers that the colour presented to him is blue. Varying degrees in which the process of discrimination may have been helped by such inferences have probably contributed in some measure to the differences of variability of the different races, and since such help from inference is almost certainly more frequent the more civilised the community, the large coefficient of the English observers is probably due in part to this cause, but such assistance from inference will not account for the fact that one man sees as red a glass of 10 units and another only one of 100 units, and the large coefficient of the English observers for red is certainly due in great measure to variability in the actual sensitiveness for this colour.

Colour-fields. Observations were made on the nature of the colour-vision of the peripheral retina. The same method was used as in Torres Straits, small coloured discs pasted on black backgrounds being passed from outside the limits of the field of vision to the point of fixation. In Torres Straits the method gave very definite results, the colour-fields being of considerable size, while differences in the limits of the fields for different colours were clearly demonstrated.

The Todas were able to preserve accurate and steady fixation but the colour-fields were in all cases very small. In many cases no colour was recognized till the patch almost reached the fixation point. Occasionally the fields were a little larger, but always much smaller than those found in Papuan or European subjects. The fields were so small that it was impossible to detect any definite differences in the sizes of the fields for different colours. Only in one man did the field for blue seem to be larger than those for other colours. This man and one boy called green *ársena* (yellow), and one man gave the same name to red in the periphery and only recognized the true colour when it reached the centre. These instances are too few to carry much weight, but it is noteworthy that they are in agreement with the behaviour of the normal European retina.

It is possible that this failure to name colours when exposed to the peripheral retina was due to some other cause than insensitiveness of this region, but considering that the Todas examined were more intelligent than the Murray Islanders and in general quite as good observers, the failure probably points to a definite sensory defect.

I have so far dealt with the individuals whose colour-vision conformed to the normal standard found among peoples of low culture. These people present the same general features which I have previously described as existing among Papuans, Egyptian fellahin, and the inhabitants of an Indian jungle. Of these features the most important is the existence of a certain lack of discrimination of differences of colour between green, blue, and violet, and a high threshold for blue as compared with red and yellow. It is open to question whether this relatively high threshold for blue is due to actual insensitiveness or merely to lack of interest, but I have little doubt that the former is the case. If so, the Todas furnish another example of a people whose defect in the nomenclature of blue is accompanied by a certain defect of sensitiveness for this colour. The Todas now constantly use a borrowed word for blue, though they still frequently confuse blue and black in nomenclature, and their threshold for blue approached more nearly to the European standard than in the case of the Papuans and Egyptians, whose nomenclature was still more defective.

Colour-blindness. I have left till last the most interesting feature of Toda colour vision, viz. the great frequency of colour-blindness. I examined 503 individuals out of a total population of about 800. Of these 320 were males of whom 41 or 12·8 °/₀ were colour-blind. Among the females, 183 in number, only two cases were found or 1·1 °/₀. The 43 individuals were perfectly typical examples of red-green blindness, conforming, so far as I could tell with the means at my disposal, to this condition as found elsewhere. In addition, there were a certain number of suspicious cases in which it was impossible to say definitely whether there was weakness of vision for red and green or whether there was only that marked degree of defective colour nomenclature which, as I have shown elsewhere[1], may simulate colour-blindness.

Every individual was first examined with Holmgren's wools, seven test wools being used, viz. red, green, pink, pale green, yellow, blue, and violet. Every individual whose matches were in any way suspicious was next examined with Nagel's card-test, which consists of coloured patches so designed as especially to deceive the colour-blind. The names of Rothe's series of coloured papers were also obtained; and in many cases observations were made with Lovibond's tintometer.

Every one of the 43 cases was a perfectly definite example of red-green blindness. Any one of them might have been taken as a demonstration-case of the characteristic mistakes made by the colour-blind. The following are examples of such confusions:—with Holmgren's bright red test-wool were matched browns and dark or dull greens; with Holmgren's pale green test were matched browns and greys, occasionally pale violets and rarely pinks; with a more saturated green were put browns and occasionally reds; the pink test-wool was matched with violets, blues, and blue-greens; a yellow wool was matched correctly in general, though often browns or very greenish-yellows were chosen, and occasionally a salmon-coloured wool; with a blue wool, were put blue-greens, violets and pinks; with a violet test, blues and pinks were chosen.

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[1] See especially *Journ. Anthrop. Inst.* 1901, Vol. xxxi. pp. 236–7.

Yellows and blues were never confused with one another, nor were red or green wools containing an element of yellow matched with those containing any blue. Thus the very common confusion made by normal Todas who matched a red with pink did not occur with the colour-blind. An exception to the statement just made is perhaps to be found in the fact that a few colour-blind individuals matched or compared a faint pink with Holmgren's pale green test. This test-wool contains a tinge of yellow, while the pinks matched contain a small amount of blue, but the coloration of the wools compared would probably be so slight to the colour-blind that they might easily put the two together. Those who made this confusion were tested with a more strongly coloured yellowish-green wool and in no case was this confused with a pink or bluish wool. All those who made matches or comparisons with Holmgren's wools symptomatic of colour-blindness confirmed the diagnosis by their errors in naming colours. The bright red paper of Rothe was often named correctly, but the green paper was usually called either red or white. Various brown papers were often called red. A bright yellow paper was nearly always correctly named, and blue was called blue or black. Greys and dull blacks were sometimes called red.

With Nagel's test the naming was far more defective. Of the pink dots, the darker were often called black and the lighter white, but either might occasionally be called green. The greenish and yellowish dots were often called red. Yellow and blue were never confused in nomenclature except when both were called red. Some individuals called nearly all the dots red. One boy called all the darker pink, green, yellow and blue dots red, but called the lighter pinks white. This preference for red as a colour-name among the colour-blind was common both with Nagel's test and with the more saturated papers, and was probably due to the more fixed character of the word in the nomenclature of the language.

Several colour-blind individuals named colours largely by comparison with natural objects; thus several compared a red to bluish tattoo marks, and two men compared green to the colour of the moon. One man compared all the large coloured papers to natural objects and in each case with approximate correctness, but with Nagel's cards he made definite mistakes, calling some pinks leaf-coloured and others indigo-coloured. Another man compared grey and green to freshly drawn blood and a dull brown to the rhododendron flower.

Many of those whose matches and names showed them to be colour-blind were tested with Lovibond's tintometer; owing to their defect the ordinary method of obtaining a threshold is rendered impossible, the different colours being confused with one another when the glasses used are very strongly coloured up to 500 or 800 units. The instrument is useful in the diagnosis of colour-blindness and I usually employed it in cases in which the other tests left any doubt.

In addition to the 43 definite cases of colour-blindness there were a certain number of others in which there may have been some weakness of the red-green sense. I have elsewhere pointed out that confusions may occur with Holmgren's wools of the same kind which occur in red-green blindness but due to a different cause. The most marked confusion of this kind is between pink and violet. The red-green blind man confuses these colours because he is blind to the red in each and probably sees both colours as blue. The Papuan and the normal Egyptian or Toda confuse the two colours because they are insensitive (or indifferent) to the blue element in each. Confusion between pink and violet occurred occasionally among the 460 Todas who were regarded as normal but less frequently than among the Egyptians or among the Uralis and Sholagas. Occasionally, however, a man went further and compared a blue wool with the pink test. Those who did this were subjected to a rigorous examination and reexamination and little doubt was left in each case that the comparison had been due to momentary lapse of attention or some cause other than colour-blindness. In one or two cases it is possible that there was some weakness for red and green. Thus, one man matched all wools well except that he put one violet wool with the pink test. With Nagel's test, he called some of the pink dots *nil*. When examined a second time with the wools, his matches were perfectly normal, and on this occasion he named all the pinks of Nagel's test correctly except one. There was no defect of general intelligence, and his mistakes on the first occasion of testing possibly had their basis in a slight degree of weakness of the colour sense which could not be detected on the second occasion.

The tests employed did not enable me to distinguish with any degree of certainty between the two chief varieties of red-green blindness, but there was much reason to believe that many of the cases were of the kind in which the red end of the spectrum is relatively darker than to the normal eye. The colours generally confused with a brilliant red were dark brown and green, and a dull black paper was often called red, showing that red must have been seen as a dark colour. Similarly with Nagel's cards the darker pinks were often called black.

The problem is complicated by the general tendency to call blue black, but this cannot explain why a black should be called red, and it is probable that many, if not the majority of the cases belonged to the group in which the red end is darker than to the normal eye.

It is perhaps significant that the Todas who have so large a percentage of their community colour-blind should have, even when colour-vision is normal, a smaller area of the retina sensitive to colour than is usually found. The association suggests the possibility that the frequency of red-green blindness may have come about by a gradual diminution of the area sensitive to red and green, and that in a large percentage of the population the area has diminished till it has disappeared altogether.

In Europeans the central area of the retina sensitive to red and green varies considerably in size in different individuals. So far as I know, it has never been suggested that red-green blindness may be only an accentuation of this variation in which the diminution has been so great as to lead to disappearance, but the association above-mentioned suggests that there may be some relation between diminution of the normal area sensitive to red and green and definite colour-blindness.

Among the Todas, however, the limitation of the field applies to all colours, not only to red and green, and a further difficulty in the way of this suggestion is that the colour-blindness of the peripheral retina of the normal eye is dependent on intensity. The periphery is only colour-blind with colours of moderate luminosity and all colours are seen if they are made sufficiently bright. In red-green blindness, on the other hand, increase of luminosity of a colour does not, in most cases at any rate, enable the colour to be discriminated.

I have already said that any one of the colour-blind Todas might have been used to demonstrate the characteristic features of red-green blindness. The mistakes were more gross than is usual with European subjects; the confusions were exaggerations of those made by the latter. I ascribe this difference to the fact that most of those tested in civilised countries are to some extent aware of their defect and try to hide or minimise it as much as possible. The colour-blind Todas, on the other hand, seemed to be unaware that they differed in any way from their fellows, and made their matches and comparisons in a free and unconstrained manner, which showed the nature of the defect more clearly than is customary in people in higher stages of culture[1].

There is another factor which may possibly have contributed to the gross nature of the confusions. Many of the colour-blind people called blue and violet *kârthiti* or black, and gave other indications that there was deficiency in the discrimination of blue in addition to the defect in red and green.

So far as we can tell from the present state of our knowledge a person suffering from red-green blindness sees all colours as shades or tints of two colours, probably yellow and blue. He is usually able to recognize that colours which normal people distinguish as red and green are also different to him, and, if he is an acute observer, he may recognize that the differences are in brightness and saturation, not in colour-tone. He is thus often able to name reds and greens correctly, though always liable to error. When being tested for colour-blindness he avails himself to the utmost of the indications by means of which he distinguishes the colours to which he is insensitive, and usually succeeds in avoiding the grosser errors with Holmgren's wools.

The Toda, on the other hand, unaware of his defect and never having paid especial attention to colour-discrimination, matches in a naïve manner, while one of the two colours on which he has to rely is probably of less use to him than to the European for he is accustomed, owing to actual sensory deficiency or for some other reason, to confuse this colour with black. It is perhaps noteworthy that the man who succeeded in avoiding several of the grosser errors in matching, though undoubtedly colour-blind, had a special reputation among the Todas for mental ability.

There was no evidence that the colour-blind individuals were defective in any other way. There was certainly no lack of general intelligence. Most of the colour-blind people were tested in various ways and there were none who had any difficulty in understanding and responding to other tests. Of six Todas who were regarded by their fellows as preeminent in intellectual power two were colour-blind.

Similarly there was no evidence of other sensory defects accompanying the colour-blindness. The average visual acuity of 18 colour-blind Todas was 2·03, only slightly less than the average for the whole number tested, and there was no obvious difference between colour-blind and normal individuals in the observations on the other senses.

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[1] My own observations on colour-blindness in England have been made on educated persons. It is possible that the less instructed might show the same gross defects as the Todas.

I was able to trace out the relationship of most of the colour-blind individuals to one another, but since this aspect of the subject is of biological rather than psychological interest, I reserve the publication of the data bearing on the point for another place.

Visual illusions.

Quantitative observations were made to ascertain the degree in which the Todas are subject to two geometrical-optical illusions; that which depends on the erroneous estimation of vertical as compared with horizontal lines and the Müller-Lyer illusion.

The illusion of compared horizontal and vertical lines. This was tested in two ways. In one the method was the same as that adopted in Torres Straits[1]. The subject of the test had to draw a vertical line from the middle point of a given horizontal line 100 mm. in length and had to make the vertical line of such a length that it appeared to him to be equal to the horizontal line; secondly, he had to draw a vertical from one end of the horizontal line; and thirdly, to draw the vertical through the middle of the horizontal so as to form a cross of which the vertical and horizontal limbs should appear equal to one another. This procedure was repeated twice and the mean of the three vertical lines was taken in each case.

TABLE V.

Estimation of Vertical Lines. Horizontal Line 100 mm.

	No.	No. 1	No. 2	No. 3
Todas....................	20	75·7	87·6	95·4
Murray Island men...	20	65·7	77·0	90·1
,, ,, boys...	12	79·5	84·3	99·4
English adults.........	15	89·0	92·5	94·5
,, children......	12	78·2	88·7	90·7

The results are given in Table V, together with those of English and Papuan subjects tested by exactly the same method. The figures give the average lengths in millimetres of the vertical lines drawn by each group.

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[1] See *Reports*, vol. II. p. 108.

It will be seen that the figures of the Todas lie between those of the Papuans and English adults, but differ very little from those of the English and Papuan children, at any rate in the first and second forms of the measurement. The Todas agree with the other groups tested in making the vertical shortest when erected from the middle of the horizontal line, *i.e.* the illusion is greatest in this form of the test. Similarly the Todas agree with the others in having the smallest illusion when the completed figure takes the form of a cross, while the figure in which the vertical is erected at one end of the horizontal line gives a result intermediate between the other two.

The same illusion was tested by a new and more satisfactory method, using a simple instrument devised by Mr Horace Darwin and myself[1]. The apparatus consists of a black board on which lie two white threads at right angles to one another. By means of an arrangement at the back of the board each of the threads can be made to vary in length from 20 to 150 mm. The board is held vertically by the person who is making the measurement, so that his line of vision cuts the board at a right angle, thus avoiding any error due to projection such as is possible in my earlier method.

In one series of observations the board was given with the horizontal line 100 mm. in length and the vertical line at its maximum length of about 150 mm., and the observer was asked to shorten the vertical line till it appeared to him to be of the same length as the horizontal. Fine adjustment was allowed when the lines were judged to be approximately equal. Five such measurements were taken and then the board was given with the vertical at its minimum length of about 20 mm., and the observer was told to make it longer till it appeared equal to the horizontal line, and five measurements were again taken.

A further series of observations was then made with a standard vertical line 100 mm. in length, while the horizontal was varied. The board was first given with the horizontal line long so that the horizontal was made equal to the vertical by a process of shortening, and when this had been done five times the horizontal was lengthened five times till apparently equal to the vertical.

After this test had been given to 20 Toda men, the procedure was reversed for the succeeding 20 individuals. The board was first given with the vertical line as the standard, the horizontal being the variable, and the observations in which the variable was made equal by lengthening preceded those in which it was made equal by shortening. In the following tables the observations made by the first procedure are marked A; those made by the second procedure, B.

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[1] *Journ. Physiol.* 1902, Vol. XXVIII.; *Proc. Physiol. Soc.* p. xl.

In these tables all the results are given in the same order, but it must be remembered that in procedure B the observations with the vertical standard preceded those with the horizontal standard, and the observations beginning with the variable short preceded those beginning with it long. Table VI gives the figures of 20 Todas tested by procedure A, and Table VII those obtained by procedure B.

In the 3rd column of each table are given the averages of the five observations made by shortening the variable vertical thread; in the 5th column those obtained by lengthening the vertical, each being followed by the mean variations of the five observations. In the 7th column are given the averages of the whole ten observations of each individual. In the second half of the table are given the corresponding figures with vertical standard and horizontal variable, beginning with the average of the whole ten observations.

If the two sets of observations are consistent with one another the measurements of the horizontal in the second half of the table should bear the same ratio to 100 as 100 bears to the measurements of the vertical in the first half of the table. Thus if to Karseidi, a horizontal line 105·2 mm. in length appears equal to a vertical line of 100 mm., a vertical line which appears equal to a horizontal line of 100 mm. should be 95·1 mm. in length. The actual measurement made by Karseidi was 94·9, showing a very close correspondence between the calculated and the observed figures.

In order to show how far the results are consistent, I have given in the 8th column of the table the values of the vertical which might have been expected on the basis of the measurements of the horizontal line when the vertical was the standard. It will be seen that in many cases there is a close correspondence, while in others there is a wide divergence, but the means of the whole twenty observers of each table show a fairly close correspondence with one another.

The general results brought out by these two tables are:

(i) That when the Todas tried to make a vertical line equal to a horizontal line of 100 mm., they made the vertical considerably too short, the average measurement of the whole 40 Todas being 93·3 mm. They had an illusion of 6·7 %. When they tried to make a horizontal line equal to a vertical of 100 mm. the illusion was somewhat greater.

TABLE VI. *Illusion of compared Horizontal and Vertical Lines.* Procedure A.

	Age	Horizontal Standard 100 mm.						Vertical Standard 100 mm.				
		Begin long	m.v.	Begin short	m.v.	Average of 10	Calculated value	Average of 10	Begin long	m.v.	Begin short	m.v.
Karseidi	59	93·8	2 96	96·0	2·00	94·9	95·1	105·2	105·2	3·36	105·2	2·64
Karsüln	58	86·6	2·32	88·6	2·08	87·6	87·5	114·3	116·2	5·24	112·4	2·48
Timurvan	50	96·4	2·72	96·2	2·64	96·3	94·0	106·4	101·8	1·36	111·0	2·80
Kuḍrvas	48	91·2	5·20	89·2	1·44	90·2	90·1	111·0	109·6	2·88	112·4	4·08
Kutaḍri	47	87·8	5·76	83·8	2·56	85·8	86·3	115·9	116·8	4·24	115·0	3·60
Monguḍrvan	44	96·2	2·24	95·4	3·68	95·8	94·1	106·3	105·0	5·20	107·6	5·12
Pepners	43	86·0	2·00	84·8	2·64	85·4	88·6	112·9	114·6	2·32	111·2	2·56
Kòḍrner	42	95·0	3·20	95·2	1·84	95·1	96·4	103·7	104·0	1·60	103·4	0·88
Ilgievan	41	99·4	1·92	103·0	2·00	101·2	92·5	108·1	107·8	1·12	108·4	0·88
Todars	40	96·0	1·60	94·0	3·20	95·0	91·5	109·3	109·4	2·16	109·2	3·04
Keitas	37	89·2	3·04	87·2	1·44	88·2	88·6	112·9	110·4	4·88	115·4	2·08
Timners	34	97·2	1·84	93·0	4·00	95·1	92·8	107·8	112·4	2·08	103·2	1·84
Kakar	33	89·0	4·00	82·6	4·08	85·8	81·1	123·3	123·6	3·92	123·0	1·20
Pepob	31	84·4	1·68	83·2	2·64	83·8	94·5	105·8	110·8	4·16	100·8	2·16
Perol	30	87·4	1·92	87·2	5·36	87·3	88·6	112·9	113·4	3·44	112·4	2·96
Polpush	27	93·8	1·52	90·6	1·68	92·2	91·6	109·2	113·8	3·44	104·6	2·48
Kerkwòḍr	22	92·8	2·64	86·6	2·72	89·7	81·3	123·0	125·0	0·80	121·0	1·20
Keinoz	21	100·4	2·72	98·6	1·68	99·5	94·5	105·8	104·6	1·68	107·0	1·60
Tilipa	20	94·6	1·68	98·0	3·20	96·3	82·6	121·0	119·6	3·44	122·4	3·12
Pundu	18	99·8	3·04	97·0	2·40	98·4	89·7	111·5	112·4	3·28	110·6	2·96
Mean	37·3	92·9	2·70	91·5	2·66	92·2	90·1	111·3	111·8	3·03	110·8	2·48

TABLE VII. *Illusion of compared Horizontal and Vertical Lines.* Procedure B.

	Age	Horizontal Standard 100 mm.						Vertical Standard 100 mm.				
		Begin long	m.v.	Begin short	m.v.	Average of 10	Calculated value	Average of 10	Begin long	m.v.	Begin short	m.v.
Perner	61	97·2	1·04	96·8	3·04	97·0	91·8	108·9	109·8	1·44	108·0	4·40
Kuriolv	54	97·4	1·68	90·4	1·92	93·9	94·2	106·2	108·0	0·80	104·4	1·12
Ivievan	48	92·8	2·64	91·2	2·16	92·0	90·3	110·8	109·2	1·04	112·4	5·52
Muḍriners	45	97·4	1·28	92·2	1·36	94·8	86·1	116·2	112·8	3·76	119·6	1·68
Toasners	40	93·0	2·40	91·2	2·56	92·1	91·6	109·2	109·8	1·84	108·6	3·28
Mongeithi	36	95·4	1·28	92·4	1·68	93·9	94·4	105·9	107·0	1·60	104·8	0·96
Persgas	36	86·6	2·56	87·8	3·76	87·2	87·7	114·0	115·8	5·84	112·2	4·96
Pungusivan	35	95·6	1·52	95·2	2·64	95·4	93·9	106·5	107·2	1·84	105·8	2·16
Kurazners	31	93·4	2·48	90·4	1·28	91·9	88·6	112·9	112·6	2·88	113·2	4·56
Mòsaḍr	31	99·0	3·60	95·4	2·88	97·2	94·1	106·3	108·6	2·72	104·0	0·80
Poiténers	30	85·4	3·28	91·4	3·92	88·4	92·9	107·6	110·6	3·28	104·6	2·88
Taḍrners	29	97·2	1·44	93·8	3·04	95·5	95·1	105·2	108·0	4·00	102·4	1·76
Arsguḍr	27	97·2	1·44	98·2	1·76	97·7	87·4	114·4	116·0	4·40	112·8	1·92
Tòthikeir	25	102·0	2·40	94·2	2·64	98·1	92·0	108·7	110·0	2·40	107·4	1·84
Kuderson	24	96·4	5·52	95·0	2·80	95·7	94·4	105·9	104·0	0·80	107·8	1·52
Polgar	24	99·4	3·52	95·6	2·08	97·5	89·8	111·4	114·8	1·04	108·0	2·80
Kalgeners	22	95·8	3·04	96·4	1·44	96·1	90·5	110·5	111·4	4·08	109·6	1·84
Kerponers	21	99·4	2·88	94·0	0·80	96·7	92·6	108·0	110·4	2·32	105·6	1·52
Elkutl	17	96·4	1·68	88·0	3·60	92·2	90·2	110·9	109·4	2·48	112·4	5·52
Keirodi	16	93·0	3·20	98·2	1·84	95·6	96·9	103·2	104·0	2·80	102·4	1·04
Mean	32·6	95·8	2·44	93·1	2·36	94·4	91·7	109·1	110·0	2·57	108·3	2·60

The average measurement in this case was 110·2 or 10·2 % of the length which would have been really equal to the vertical.

The reason for the difference with the two standards will be discussed later.

(ii) On comparing the observations in which the variable line is made equal by a process of shortening with those in which it is lengthened, it will be seen that the measurements are somewhat greater in the former case. The difference occurs with both standards and both procedures and in the majority of the individuals; in 31 out of 40 with the horizontal standard and 27 out of 40 with the vertical standard. The difference is not great, but is of the same kind as is usually found in such measurements[1].

(iii) The observations with the two standards are fairly consistent with one another. As we have seen already, the illusion is apparently greater with the vertical standard, and corresponding to this the calculated value of the vertical is smaller than the observed value, the figures for the whole 40 subjects being 90·9 and 93·3 mm. In 21 out of the 40 subjects the difference between the two values is less than 3 mm., and often the correspondence is very close.

In those cases in which the two values diverge widely from one another we may conclude either that the subjects were careless, or that for some reason their standard of measurement altered, or that the measurement of the illusion was complicated by some other factor. As we shall see shortly, in similar observations made by English subjects, such a complicating factor is undoubtedly present.

(iv) Age has no appreciable influence on the degree in which the illusion is experienced. In Table VII those above 35 have slightly the greater illusion, 93·3 as compared with 95·2 and 109·7 as compared with 108·8, but in Table VI there is no appreciable difference with the horizontal standard, while with the vertical standard the younger group has the greater illusion.

In Tables VIII and IX are given the observations of 40 English men, Cambridge graduates and undergraduates, arranged in exactly the same way as those of the Todas. It will be seen that the illusion is much smaller in amount, and in the first set of observers with the horizontal standard it appears to be completely absent, the vertical line being made on the average 100·2 mm. in length in order that it should appear equal to a horizontal line 100 mm. in length. On going through

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[1] See p. 359, and Torres Straits *Reports*, Vol. II. p. 120.

TABLE VIII. *Illusion of compared Horizontal and Vertical Lines.* Procedure A.

	Horizontal Standard 100 mm.						Vertical Standard 100 mm.				
	Begin long	m.v.	Begin short	m.v.	Average of 10	Calculated value	Average of 10	Begin long	m.v.	Begin short	m.v.
T. A. W.	111·8	1·72	103·0	1·20	107·4	106·2	94·2	93·8	1·84	94·6	3·12
A. G. A.	104·0	2·00	103·2	1·04	103·6	98·1	101·9	104·0	1·60	99·8	0·64
L. W. S.	105·2	1·84	101·8	0·64	103·5	93·6	106·8	108·0	1·60	105·6	0·48
G. H. D.	105·8	1·04	100·2	1·76	103·0	97·5	102·6	106·0	1·20	99·2	1·84
W. C. D.	104·4	3·52	101·4	2·32	102·9	94·3	106·0	105·4	1·92	106·6	1·84
P. P. L.	104·6	1·68	100·2	1·36	102·4	99·2	100·8	100·0	0·80	101·6	0·88
M. S. M.	101·6	1·68	102·6	2·48	102·1	99·7	100·3	102·2	2·16	98·4	1·92
A. E. S.	101·8	3·20	102·4	0·48	102·1	99·7	100·3	101·0	0·80	99·6	1·12
C. E. W.	106·2	1·92	96·6	1·28	101·4	98·0	102·0	105·0	3·20	99·0	2·80
A. G. S.	103·4	0·88	98·6	1·92	101·0	95·9	104·3	110·0	2·80	98·6	2·88
T. S. H.	102·6	0·72	98·4	1·28	100·5	97·4	102·7	105·6	1·12	99·8	0·32
G. L. S.	100·6	1·84	100·2	0·64	100·4	98·4	101·6	102·8	2·48	100·4	1·12
H. N. W.	102·8	2·24	96·2	2·16	99·5	90·8	110·1	110·0	1·20	110·2	0·64
G. W. T.	99·2	1·12	97·8	1·04	98·5	91·2	109·6	110·8	1·44	108·4	0·48
W. J. P.	96·4	1·68	97·8	1·04	97·1	97·0	103·1	101·6	0·48	104·6	1·36
O. M.	99·2	0·72	94·4	1·28	96·8	94·6	105·7	109·0	2·80	102·4	1·28
P. C. V. J.	96·6	0·88	95·6	0·88	96·1	92·8	107·8	112·0	3·60	103·6	1·68
D. T. J.	99·0	2·40	93·0	1·20	96·0	88·3	113·2	110·4	1·68	116·0	2·80
H. F. L.	95·4	2·72	93·8	0·72	94·6	93·1	107·4	108·0	2·00	106·8	1·04
H. W. H.	96·6	1·92	92·0	2·00	94·3	93·7	106·7	106·6	1·84	106·8	1·84
Mean.........	101·9	1·79	98·5	1·34	100·2	96·0	104·4	105·6	1·83	103·1	1·50

TABLE IX. *Illusion of compared Horizontal and Vertical Lines.* Procedure B.

	Horizontal Standard 100 mm.						Vertical Standard 100 mm.				
	Begin long	m.v.	Begin short	m.v.	Average of 10	Calculated value	Average of 10	Begin long	m.v.	Begin short	m.v.
J. L.	106·2	1·44	102·4	2·32	104·3	95·2	105·0	106·2	2·64	103·8	0·96
S. C. L.	104·0	0·80	101·8	1·04	102·9	94·9	105·4	107·2	1·86	103·6	1·52
F. W. E.-G. ...	104·0	3·20	100·4	2·08	102·2	96·4	103·7	106·8	1·04	100·6	0·48
H. B. C.	101·0	0·40	97·2	1·44	99·1	101·6	98·4	99·2	2·64	97·6	2·08
P. H.	100·6	1·84	97·2	4·64	98·9	99·4	100·6	101·2	2·24	100·0	1·20
E. B.	99·6	0·72	97·8	3·76	98·7	94·2	106·1	107·4	1·68	104·8	2·16
L. J. P. J.	100·2	3·04	96·6	0·88	98·4	99·8	100·1	103·8	2·96	96·4	2·32
E. G. F.	99·2	2·24	96·6	1·36	97·9	97·8	102·3	103·2	2·96	101·4	1·12
J. S.	97·4	1·12	98·0	2·40	97·7	91·7	109·1	111·8	2·16	106·4	1·12
R. C. P.	98·4	1·52	96·8	0·72	97·6	97·8	102·2	100·8	0·64	103·6	1·68
W. D. H.	98·2	1·84	96·4	2·72	97·3	100·7	99·3	101·8	0·64	96·8	1·44
E. M.	97·2	1·84	96·8	2·24	97·0	95·1	105·2	105·6	1·68	104·8	0·64
A. E. C.	97·6	1·12	96·2	0·64	96·9	90·6	110·4	111·8	2·48	109·0	1·20
R. T. C.	97·8	0·64	94·2	1·44	96·0	94·2	106·1	106·2	1·84	106·0	1·60
V. H. M.	95·2	1·36	95·0	1·20	95·1	94·4	105·9	106·0	1·20	105·8	1·44
J. L. J.	99·0	1·20	90·6	1·52	94·8	87·6	114·2	117·8	0·96	110·6	1·12
A. T. D.	92·4	0·88	95·8	0·96	94·1	90·4	110·6	109·4	2·88	111·8	3·28
W. H. C.	93·0	0·04	93·0	1·20	93·0	97·9	102·1	106·0	1·20	98·2	2·16
A. C. H. R. ...	94·6	1·52	90·4	0·88	92·5	92·8	107·8	113·6	1·68	102·0	3·20
E. H. H.	91·4	1·68	90·8	0·64	91·1	91·7	109·1	111·8	1·86	106·4	0·88
Mean......	98·4	1·42	96·2	1·70	97·3	95·2	105·2	106·9	1·81	103·5	1·58

the figures of this table in detail it will be seen that 12 out of the 20 observers made the vertical line longer than the horizontal. With the second set of observers whose figures are given in Table IX, there is distinct evidence of the illusion, and only three out of the twenty made the vertical line longer than the horizontal standard.

On passing to the observations with a vertical standard the existence of the illusion comes out definitely, the variable horizontal being made on the average 104·4 mm. in length by the first set of observers, and 105·2 by the second set, and only three out of the whole 40 people had an average horizontal shorter than the vertical standard.

In consequence of this difference between the measurements with the two standards there is a decided difference between the observed and calculated values of the vertical, especially in the first set of observers, the difference in the second set of English observers being about the same as in the case of the Todas. In all four tables the differences are in the same direction.

The figures of the English observers show exactly the same differences depending on the direction of adjustment of the variable line. The differences are somewhat greater on the average for the English than for the Toda observers and are rather more constant, occurring in 34 out of 40 with the horizontal standard and in 31 out of 40 with the vertical standard.

In the first four lines of Table X are given the averages taken from the preceding tables, and I have added the results of observations made on English women and children by Miss A. Hingston, who has kindly allowed me to give them here for purposes of comparison. Miss Hingston's observations were made with the same apparatus and by exactly the same methods as my own. The women were students of Homerton Training College at Cambridge, and the children, who were of both sexes, belonged to the village of Flax Bourton in Somersetshire.

In the last line I have added the figures for 14 Uralis and Sholagas who were only examined with a horizontal standard.

According to this table all the English observers agree in showing either a very slight or no illusion with the horizontal standard, while the illusion with the vertical standard is distinctly smaller than for the Todas. The differences between the figures for the Todas and the English observers are very much larger than the probable errors of the differences. The probable error of the difference between the Todas and Englishmen with the horizontal standard by procedure A is ·92, and the other probable errors between Todas and Englishmen are smaller than this.

TABLE X.

	No. examined	Horizontal Standard 100 mm.						Vertical Standard 100 mm.				
		Begin long	m.v.	Begin short	m.v.	Average	Calculated average	Average	Begin long	m.v.	Begin short	m.v.
Todas, A.	20	92·9	2·7	91·5	2·7	92·2	90·1	111·3	111·8	3·0	110·8	2·5
Todas, B.	20	95·8	2·4	93·1	2·4	94·4	91·7	109·1	110·0	2·6	108·3	2·6
English men, A.	20	101·9	1·8	98·5	1·3	100·2	96·0	104·4	105·6	1·8	103·1	1·5
English men, B.	20	98·4	1·4	96·2	1·7	97·3	95·2	105·2	106·9	1·8	103·5	1·6
English women, A....	25	100·8	2·3	97·8	1·6	99·3	96·6	103·7	104·7	2·1	102·8	1·3
English women, B....	25	98·9	1·6	97·7	1·6	98·3	96·4	103·6	105·3	2·1	102·0	2·2
English children, A..	25	102·7	2·6	99·9	2·9	101·3	95·1	105·5	106·9	4·0	104·1	3·7
English children, B..	25	103·5	3·2	97·8	3·5	100·7	94·4	106·9	109·5	3·2	104·3	3·9
Uralis & Sholagas, A.	14	93·9	3·0	92·2	2·9	93·1	—	—	—	—	—	—

In all the English groups there is the same kind of relation between the calculated and observed values of the vertical. The former is always smaller than the latter, though the amount of the difference varies. It seems certain that some disturbing factor has come into the observations which has led to the discrepancy and has produced an appearance of no illusion with the horizontal standard. In the English children, in whom the difference between the observed and calculated values of the vertical is greatest, the vertical variable is, on the average, made distinctly longer than the horizontal standard.

All these discrepancies would be explained if there should exist a general tendency to make the variable line too long when making one line equal to another. If there is a constant or general tendency to make the variable too long, the result would be to mask the illusion when the vertical is the variable and to accentuate the illusion when the horizontal is the variable, and the calculated value of the vertical would come out less than the observed value.

In order to discover whether such a tendency to make the variable line too long really existed I tested 20 individuals, using a piece of apparatus constructed on the same plan as that employed for the measurement of the illusion. This consisted of a board covered with black cloth, on which lay two white threads in a line with one another, each of which could be made of any desired length by means of an arrangement at the back of the board. Four series of measurements were made; in one two horizontal lines were given, of which that on the left hand of the observer, 100 mm. in length, was the standard, and the line on the right hand was adjusted till it appeared equal to the standard. Five observations were made by shortening the line, and five by lengthening, as in the observations on the illusion. In the second series of measurements the conditions were the same, except that now the right-hand line was the standard and the left-hand line was the variable. In a third series the lines given were both vertical, the lower, 100 mm. in length was the standard and the upper variable. In a fourth series the lower vertical was varied.

The results are given in Table XI, in which the figures are arranged on the same plan as in the preceding tables.

These figures show that when an attempt is made to adjust the length of one line so as to make it equal to another there is a distinct tendency to make the variable line too long. The error is not large but it occurs very frequently among the twenty individuals tested. Thus in the first series, 16 out of the 20 individuals made the right horizontal line too long. In the other three series the numbers were 13, 12, and 14 out of 20.

TABLE XI.

Variable lines	Average of 10	Begin long	m.v.	Begin short	m.v.
Right horizontal ...	102·3	103·5	1·68	101·2	1·44
Left horizontal.. ...	101·6	102·3	2·04	100·9	1·53
Upper vertical	100·9	101·3	2·19	100·5	1·66
Lower vertical	102·1	102·3	1·73	101·9	1·74

Many of those tested were the same individuals as those of Tables VIII and IX. If we assume that in adjusting the upper vertical of the illusion-figure they made the same errors as when comparing upper with lower vertical, and that in adjusting the right horizontal of the illusion they had the same tendency to error as when comparing right with left horizontal, we may make the corrections suggested by the figures in Table XI. The result would be that in the A observations of the English men, the average with horizontal standard would become 99·3 and that with vertical standard 102·1. In the B observations the figures would become 96·4 and 102·9. These figures are still not quite consistent with one another but they show clearly that corrections of the kind suggested by the results given in Table XI would bring the two series into harmony with one another, and they leave little room for doubt that the apparently greater amount of the illusion with the vertical standard is due to the fact that the process of making one line equal to another according to the method adopted is complicated by a general tendency to make the variable line too long[1].

The corrected figures show that there is a distinct illusion in the case of the English men; that to them a vertical line appears to be of the same length as a horizontal line when it is really slightly shorter, and that correspondingly a horizontal line appears of the same length as a vertical line when it is really slightly longer.

If the same corrections are applied to the figures of the English women, the average lengths of the lines would come out 98·4 and 101·4 in the A procedure; 97·4 and 101·3 in the B procedure, measurements nearly consistent with one another.

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[1] The explanation of this general tendency is doubtful. It cannot be due to the direction of movement, for the directions are reversed after every five observations and exactly the same number of observations are made in each direction.

The figures for the children as given in Table X are the most inconsistent of the three English groups and seem to show a total absence of the normal illusion. If the same corrections are applied to them as in the above cases, the figures come out 100·4 and 103·2 in the A procedure; 99·8 and 104·6 in the B procedure. They are still very inconsistent with one another and suggest that the general tendency to make the variable too long was stronger in the children than in the adults[1].

Since the Todas show the same kind of difference between the calculated and observed values of the vertical as the English observers we may conclude that in them also there existed a tendency to make the variable line too long, and since the difference between the two values has in their case much the same extent as in the English adults, we may perhaps be justified in regarding it as probable that they show this general tendency to much the same extent as the Englishmen. In the Todas, however, the illusion is so much greater than in the English observers that the effect of the general tendency is much less noticeable.

If the corrections I have suggested, or slightly large corrections in the case of the children, be accepted, the figures of Table X would show the existence of the illusion in the three groups of English observers, but evidently in a smaller degree than in the case of the Todas.

The smallness of the illusion shown in the observations of the English adult observers is undoubtedly due in great measure to the use of indirect means of comparing the lengths of the two lines. In the case of the men, I found that all sorts of devices were employed to assist the observer in his judgments. Some would imagine circles drawn from the point of junction of the two lines with these lines as radii; others would complete the triangle ideally and observe the angles; another would imagine each line halved and compare half of one line with half of the other and so on. Even when an observer tried to avoid such devices, he could not get them wholly out of his mind and found himself influenced by them.

It is very unlikely, on the other hand, that the Todas adopted any such definite artificial means. Many were intelligent enough to adopt indirect means, but any devices adopted would almost certainly have been far more crude than those of the English observers, and it is improbable that indirect means were employed to any great extent, the average Toda relying solely on the sensory data given by the two lines and their arrangement in space.

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[1] I have not yet been able to put this point to the test.

In the case of the children tested by Miss Hingston, the reason for the smallness of the illusion is more doubtful but it is possible that practice in drawing may in their case as in that of the adult have contributed to the result, for the illusion in question is one which everyone who has learnt to draw has had to overcome. I may mention in this connexion that the Todas, unlike the members of many more primitive races, are very bad draughtsmen.

On comparing the results given in Table X with those obtained by the earlier method given in Table V, it will be seen that with both Todas and English men, the illusion appears to be distinctly greater when tested by the earlier method in which the figure is drawn on paper. The numbers given for No. 2 in Table V apply to a figure of exactly the same form as was used with the thread apparatus, but the vertical is here made 87·6 mm. in length by the Todas as compared with 92·2 and 94·4 by the thread method. Similarly the result for English adults is 92·5 as compared with 100·2 and 97·3. It is far from clear why the illusion should come out so much more strongly when the lines are drawn on paper than when threads are adjusted by the hand. One important difference between the two procedures is that in the earlier method the figure is drawn on paper lying on a table and thus in a horizontal plane, while in the observations with the thread apparatus the board was always held in a vertical plane. In the earlier observations there might therefore be an error due to projection but it would have been expected that the influence of this factor would have been exerted in the opposite direction.

Another possible cause of difference is that in the drawing method the observations were preceded by others in which the vertical was drawn from the middle point of the horizontal line. In this procedure the illusion is considerably greater and the memory of the short lines drawn just before from the middle of the horizontal line may have influenced the succeeding observations in which the vertical was drawn from one end of this line.

It will be noticed that the figures of the women and children in Table X show the same differences according to the direction of adjustment which were found in the observations on the Todas and English men, and in the English children tested by procedure B the differences are much larger than in any other group. This difference is one of the most constant features of observations in which one line has to be made equal to another. The figures of Table XI show exactly the same characteristic though to a somewhat smaller extent than in the observations on the illusion.

It would seem that when anyone is given a long line and is asked to make it equal to a shorter line, he is influenced throughout by the fact that he has started with a long line and thinks he has reached equality when the variable line is still distinctly too long. When he starts with a short line, he is influenced in the opposite direction. Often the difference is very considerable, amounting in some individuals to as much as 10 mm. or 10 %. The difference cannot be due to any difference of time-order, for in procedure B the time-order was the opposite of that in procedure A, the observations in which a long line was shortened preceding those in which a short line was lengthened in procedure A, while this order was reversed in procedure B. Inspection of the tables shows that the difference is quite as great in one case as in the other.

The average mean variations shown in Table X, which give a measure of the consistency of the observations made by different individuals, do not differ very greatly for the Todas and the English observers. The mean variations of the Todas are greater than for the English adults, but are smaller than those of the English children.

The nature of these mean variations is complex. In the case of observations on illusions, a large mean variation does not necessarily imply carelessness or defective attention on the part of the subject. The most careful observer will sometimes show sudden variations in his measurements, due to some change in the mental attitude towards the illusion. Not infrequently a person makes two or three almost identical measurements and then two or three widely different, and so far as I have been able to learn from the introspections of my English subjects, this is due to a sudden recognition that a measurement which appears to be good at one moment may suddenly be recognized to be too high or too low. Nevertheless the most important factor upon which the mean variation depends is the amount of care and attention paid to the test, and if the Todas had fallen far short of the English adult observers in this respect it would have produced a larger difference in the variations than that shown in the table.

There is no obvious difference between the variations of the first and second five observations of each series, showing that neither practice nor fatigue had any obvious influence unless, indeed, these two possible causes of alteration neutralised one another.

In Table XII I have given the averages for the different groups of the whole ten observations of each individual together with certain other data; the maximum, medium, and minimum averages of each group; the mean variations of the individuals of each group from the average of the group; the standard deviations and the coefficients of variation.

TABLE XII.

		Average	Max.	Med.	Min.	M.V.	σ.	C.V.
Horizontal Standard	Todas, A.	92·2	101·2	93·6	88·8	4·6	5·1	5·5
	Todas, B.	94·4	98·1	95·5	87·2	2·4	2·9	3·1
	English men A. ...	100·2	107·4	100·8	94·3	2·8	3·4	3·4
	" B. ...	97·3	104·8	97·5	91·1	2·1	3·3	3·4
	English women A. ...	99·3	108·1	100·3	90·8	3·1	3·9	4·0
	" B. ...	98·3	108·1	97·9	89·4	3·5	4·4	4·5
	English children A....	101·3	113·4	101·1	89·9	4·2	5·[illegible]	5·8
	" B....	100·7	127·8	99·6	81·8	6·0	8·5	8·4
Vertical Standard	Todas, A.	111·3	123·3	110·2	103·7	4·6	5·7	5·1
	Todas, B.	109·1	116·2	108·8	103·2	2·8	3·4	3·1
	English men A. ...	104·4	113·2	103·7	94·2	3·4	4·2	4·0
	" B. ...	105·2	114·2	105·3	98·4	3·3	4·1	3·9
	English women A. ...	103·7	115·9	102·5	98·0	2·7	4·3	4·1
	" B. ...	103·6	114·4	103·3	96·1	3·5	4·4	4·2
	English children A....	105·5	122·5	106·0	93·3	4·7	6·1	5·8
	" B....	106·9	135·5	107·2	91·5	7·5	10·1	9·5

Following my procedure in the Torres Straits *Reports*, I have headed the column for the mean variation M.V. in capital letters in order to distinguish this, which is a measure of the variability of individuals, from the m.v. of Tables VI—XI, which is a measure of the variability of observations[1].

It will be seen that on the whole the Todas vary from one another to a greater extent than the English men; that they do not differ appreciably in this respect from the English women and are distinctly less variable than the children. The two groups of Todas differed considerably from one another, and it will be seen in the other columns of the table that the maximum and minimum observations fell in the more variable group. The difference is due to the inclusion in the first group of a few men whose results deviated widely from the average and would almost certainly have disappeared if a larger number of figures had been available.

Here as in the case of the tintometer results, the factors upon which the mean variations and standard deviations depend are complex, but a very important factor in this case is undoubtedly the difference in the method employed by the observers in making their measurements. There can be little doubt that the methods employed by the English observers were more complex; we have seen that many English observers made use of various devices in making the two lines equal to one another, and we can be fairly confident that these devices were not employed to nearly so great an extent by the Todas, who relied much more on purely sensory data. But, on the other hand, the various indirect methods employed by the English observers all led to the same end, viz. the elimination of the illusion and consequent approximation to equality of the measurements of the two lines, and this would of itself lead to a diminution of the mean variation and standard deviation.

I do not think there can be any doubt that in their methods of making the measurements, the English observers varied from one another more widely than the Todas, and yet the measure of variability—the coefficient of variation—comes out smaller in the former. This seems to me to be a very instructive example of the difficulties attaching to the interpretation of statistical measures of variability. Comparing the Todas and the English men, the group whose members varied most widely in their methods of measurement have yet the smaller coefficients of variation—or at any rate, they certainly do not show larger coefficients—and I do not think there can be any doubt that this is due to the fact that the variations in method of the English observers all worked in one direction, *i.e.* towards greater accuracy of measurement.

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[1] I would suggest that the same means of distinction might also be followed in the case of the standard deviation and coefficient of variation. In this paper I have only applied these measures to individual variability.

The different measures of variation of the individuals of each group from one another are undoubtedly influenced by misunderstanding, carelessness, or defective attention on the part of any individuals included in the groups. An individual who misunderstands the test or who is careless may give readings widely divergent from his fellows, and these will have their effect on the measures of individual variability. Such cases of misunderstanding or carelessness may possibly have contributed to the high measure of variation of the first group of Todas, and they are almost certainly the cause of the very large figures of the second group of English children. A few of the children included in this group made measurements which show either that they misunderstood what they were required to do or that they were extremely careless.

The Müller-Lyer Illusion. In Torres Straits this illusion was tested by means of an instrument which had two faults. The lines forming the figure were too thick, and the junction between the stationary and moveable parts of the apparatus was in the middle of the variable line, so that observers were sometimes influenced in their measurements by noticing the relative lengths of the two parts of this line.

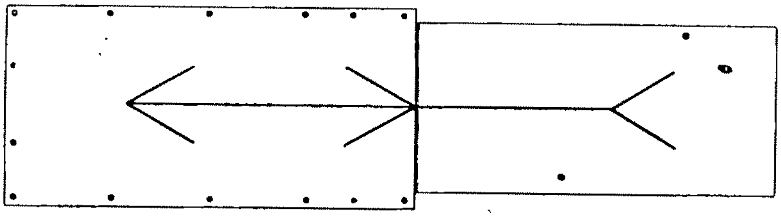

FIG. 1.

In India I used an improved apparatus (Fig. 1) made of thin xylonite. One part of the apparatus slides in and out of a framework, on the upper surface of which is drawn one half of the Müller-Lyer figure, while the other half is drawn on the moveable sliding portion. The lines of which the figure consists are only half a millimetre broad and the point of junction between the two parts of the figure corresponds with the line of junction between the two parts of the apparatus.

The method used was exactly the same as that followed with the older instrument. The observer had to make five observations by sliding the moveable part in till the two lines of the figure appeared to him to be equal to one another, and then a second series of five measurements was made by drawing the sliding part outwards till the two parts again appeared equal. In the first series, the variable line was made equal to the standard by a process of shortening, in the second, by a process of lengthening the variable line.

In order to obtain a means of comparison between the results of the Todas and those from Torres Straits a series of observations was also carried out with the old instrument. The results of both sets of observations are given in Table XIII, together with those obtained elsewhere. I do not give here the results of the individual Todas tested but have grouped the results of each set of twenty individuals together. The figures of the lines in which the names are marked with an asterisk are those obtained with the old form of instrument.

TABLE XIII. *Müller-Lyer Illusion. Standard* 75 mm.

Race	Number	Average	1st begin long	m.v.	2nd begin short	m.v.	M.V.	σ.	C.V.
Todas*	20	59·1	60·6	2·46	57·6	2·17	3·0	3·7	6·2
Todas	20	59·8	61·2	2·25	58·4	1·72	2·77	3·7	6·3
Murray Islanders*...	38	61·1	62·8	2·77	59·3	2·03	3·89	5·0	8·1
Uralis and Sholagas	28	55·3	57·2	2·46	53·4	2·36	3·52	4·2	7·7
English*.	42	55·6	57·4	2·09	53·8	1·58	5·02	6·3	11·4
English men	20	59·0	60·2	1·96	57·8	1·64	4·49	6·1	10·3
English women	25	58·5	59·4	2·06	57·6	1·48	3·27	4·3	7·3
English children ...	25	56·5	58·7	2·89	54·4	2·36	3·82	4·7	8·3
English	70	57·9	59·4	2·15	56·5	1·82	3·9	5·1	8·8

In the first place it will be seen that there is very little difference in the measurements made by the Todas with the two instruments. With the new instrument, the line terminated by diverging lines appeared to the Todas to be equal to the line terminated by converging lines, which was 75 mm. in length, when its average length was 59·8 mm. With the old instrument, the average measurement was 59·1. Thus, the illusion came out slightly less with the newer instrument. It might have been expected that the difference would have been greater owing to the greater thickness of the lines in the older instrument, but the actual difference is of the kind which might have been anticipated. In both series the observations show the characteristic difference according to the direction in which the variable line is adjusted.

In the lines of the table following the Toda figures I have given the results of the observations made on 38 Papuans of Murray Island and on 28 Uralis and Sholagas, the former with the old form of the instrument and the latter with the new.

The last compartment of the table contains the figures obtained from the observations of English subjects. Those in which the older form of instrument was used are taken from the Torres Straits *Reports*. The figures for the newer instrument were obtained from observations on the same individuals as were tested with the illusion of compared verticals and horizontals, the observations on the women and children having been made by Miss Hingston. The last line of the table gives the average figures of all the English observers tested with the newer instrument.

In the three groups of English subjects, the average illusion is slightly greater in the children than in the adults, while the men and women differ very little from one another.

The English observers show a decided difference between the measurements made with the two instruments, and this is especially the case with the adults. As might have been expected, the differences in the nature of the instrument appear to have had more influence on the more trained observers.

On comparing Todas, Papuans and English with the old instrument, it will be seen that the Todas occupy a position intermediate between the other two peoples, and if we assume that measurements made by the Murray Islanders with the new instrument would have been approximately equal to those with the old, the Todas would still occupy this intermediate position, though there is very little difference between the figures of the Todas and those of the English men. In the observations with the old form of instrument this difference is about four times the probable error of the difference, but with the new instrument it is about the same as the probable error. It would seem that the Toda sees the Müller-Lyer illusion to a somewhat greater degree than the Murray Islander, and in a slightly smaller degree than the average English observer.

In the Torres Straits *Reports* I suggested as the reason for the slighter degree of the illusion to the savage observer, the fact that he concentrates his attention more completely on the two lines which he is told to make equal and is less influenced by the figure as a whole. If this explanation be correct, and I have very little doubt that it is so, it would seem that the Toda is more influenced by the figure as a whole than the Murray Islander and almost to as great a degree as the English observer.

The figures obtained from the Indian jungle tribes, the Sholagas and Uralis, might be brought forward against this view, for these people are in a stage of culture quite as low as the Murray Islanders and it might have been expected that the two races would have given similar results. Instead of this, the figures would seem to show that the Sholagas and Uralis experience the illusion more strongly than any other group tested. In recording their observations elsewhere[1], I have given what I believe to be the explanation. In testing them I had to rely entirely on the interpreter, and I could not get him to point out the lines to be made equal with sufficient care, and I believe that the attention of these people was not directed fully to the chief lines of the figures, but that they were often trying to make the two parts of the figures similar to one another rather than to produce equality of the two lines. Owing to the very timid nature of these people and my small acquaintance with them, their observations were far less satisfactory than those of either the Todas or Murray Islanders.

The difference depending on the direction of adjustment is present and is of the same kind and approximately of the same amount in every group. It is greatest in the Indian jungle people and the English children. As in the case of the measurements already considered, this difference is the most constant feature of the experiment. It occurred in 33 out of the 38 Murray Islanders, in 24 out of the 28 Sholagas and Uralis, in 32 out of the 40 Todas, and in 92 out of the 112 English subjects. As we have seen was also the case with the illusion of compared horizontals and verticals (see p. 352), the difference is greater with the English children than with the English adults.

The mean variations (m.v.) of the observations do not show any very great differences between the different groups of the table. The average variations of the Todas are larger than those of the English adults and smaller than those of the English children.

On comparing all the groups of the tables, it will be seen that, on the whole, the Todas have smaller variations (m.v.) than the Murray Islanders or the Urali-Sholagas, and that in this respect the

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[1] *Loc. cit.* p. 18.

Todas again occupy a position intermediate between the more savage communities and the English observers. Here as in the case of the former illusion the mean variation depends on several factors, but we may safely conclude that so far as it is an index of the care and attention devoted to the measurements the Toda does not fall far short of the more highly trained English observer.

There is one feature of the mean variations which differentiates them very strikingly from those of the illusion of compared horizontals and verticals. In the case of this illusion we saw that there was no appreciable alteration of the mean variation in the different groups of each series. There was no evidence of improvement in the consistency of the observations with practice. In Table XIII, on the other hand, the mean variations in every group tested are smaller for the second than for the first set of five measurements. The only group which does not show a decided diminution in the mean variation is that of the English children. Thus, in the case of the Müller-Lyer illusion there is evidence of distinct improvement in the consistency of the observations during the series of ten observations, while there is no such evidence of improvement in the case of the illusion of compared horizontals and verticals. It would seem that the first observations of an individual on the Müller-Lyer illusion are often erratic, and that after a few observations he acquires some sort of standard or method in making the measurement whereby his observations become more consistent, and this improvement is shown alike by Todas and English observers. The failure of the English children to improve is probably due to loss of interest and increasing defect of attention in some cases during the second five observations.

In the case of the Todas, there is no very marked difference between the mean variations of the two illusions, if one takes into account the fact that the standard in the case of the Müller-Lyer illusion is only 75 mm.

In the English adults, the mean variations are smaller for the illusion of compared horizontals and verticals than for the Müller-Lyer illusion; a fact probably connected with the very small illusion in one case and the considerable illusion in the other.

The Todas have slightly larger mean variations with the older form of instrument and this, on the whole, is also true of the English adults. This result might have been expected, for the conditions with the older instrument are undoubtedly more complex.

In the last three columns of Table XIII I have given the mean variations (M.V.), standard deviations, and coefficients of variations, as measures of the variability of the different members of each group. The three measures all apply to the averages of the sets of ten observations made by each individual.

In my observations with the older form of instrument, it was found that the mean variation of the individuals from the average of the whole group was very distinctly greater for the English than for the Papuan observers, and I ascribed this to greater variability in the methods of making the comparison. Owing to their knowledge of the illusion, the whole operation became more complex for the English than for the Papuan observer, and the greater complexity showed itself in a larger measure of variability. The smaller variability of the Murray Islander was, in fact, ascribed to his ignorance, and this view was supported by the result of the comparison of English subjects acquainted with the illusion and those who had little or no knowledge of it. The former were found to have much the larger variations[1].

The observations with the new instrument show a difference of the same kind between the Todas and the English observers. The Todas have smaller mean variations (M.V.), standard deviations, and coefficients of variation than any of the groups of English observers.

The differences between the three English groups with the new instrument are strongly in favour of the view advanced above. Of the three groups, the men undoubtedly had the greatest acquaintance with the illusion and all three measures of variability are larger for them than for the women and children.

The evidence afforded by the measures of variability would seem to show that the comparison of two lines in the Müller-Lyer figures is a simpler mental operation to the savage than to the civilised person, and this is in accordance with the smaller degree in which the former is subject to the illusion, and with the view that this is due to the greater concentration of the attention of the savage on the two lines he is asked to make equal and the smaller influence on his mind of the figure as a whole.

It will be noticed that there is a very striking difference between the nature and relations of these different measures of variability for the two illusions which have been considered in this section. In the case of the illusion of compared horizontals and verticals, the English observers had much smaller measures of variation than the less civilised observers; in the case of the Müller-Lyer illusion, their variations are much greater. And yet there can be little doubt that there was in both cases greater complexity and variability in the process of comparison among the English observers. The measures of variability, however, are small in one case because all the different devices adopted led in the same direction, *i.e.* they were all designed to counteract the illusion, and since they were approximately successful the whole group of individuals have given small measures of variability.

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[1] See *Reports*, Vol. II. p. 124.

In the case of the Müller-Lyer illusion, on the other hand, though most of the English observers knew of the illusion, few, if any, had definite knowledge of its magnitude; further, there are no such obvious devices to assist comparison as in the case of the other illusion. In no case was the influence of knowledge great enough to counteract the illusion even approximately, and the result is that the greater complexity of the process in the case of the English observers has shown itself in larger coefficients of variation. In one case greater complexity has shown itself distinctly in larger measures of variability; in the other, greater complexity has led to smaller measures of variability.

Other illusions. Several other geometrical-optical illusions were shown and it was found that in general they were seen by the Todas; thus the illusion of Zöllner's pattern and other similar figures was certainly experienced. The illusion which excited the greatest interest was that numbered C_6 in the Milton-Bradley collection, in which two curved pieces of cardboard of the same size look very different when placed side by side. Many men came to me especially to see this illusion, and they experimented with the pieces of cardboard as intelligently as any European could have done. They tried the effect of placing the cards together suddenly and gradually; they tried placing them with their shorter sides in apposition and noted that the cards then appeared of the same size, and similarly, they placed the cards in every variety of position relative to one another, noting when the illusion was and was not present. When I asked them for an explanation of the appearance, some supposed that there must be some kind of magic at work, but finally two old men arrived, one of whom had a special reputation for sagacity, and after much experimenting and deliberation they gave me the correct explanation, viz. that the short side of one appears shorter than it really is because it is next to the longer side of the other, and that this gives the two cards a different appearance.

While both savage and civilised people appear to be subject to geometrical-optical illusions in general, differences in the degree to which they are subject to the illusions are shown by the quantitative observations. Only two illusions have so far been studied exactly, but of these one, the illusion of compound horizontal and vertical lines, is distinctly more marked to both Toda and Papuan than to the European. The other, the Müller-Lyer illusion, is less marked to the less civilised observers, and this more distinctly in the case of the Papuan than in that of the Toda

Although our knowledge as to the essential nature of these illusions is very defective, there is much reason to believe that they belong to two different categories. The illusion of compared horizontal and vertical lines probably belongs to that class of illusion which depends on physiological conditions, and the effect of experience in civilised life, such experience for instance as is derived from the study of geometry and drawing, is to diminish the illusion. The Müller-Lyer illusion, on the other hand, is one of those of which the explanation is probably more strictly psychological. The psychological factors upon which the illusion depends are however of a simple nature and affect both savage and civilised man, and the reason why the illusion is rather less marked to the Toda and Papuan is probably that which I have already suggested, viz. a difference in the direction of attention, the savage attending more strictly to the two lines which he is desired to make equal, while the civilised man allows the figure as a whole to exert its full influence on his mind.

The illusions so far examined are very few in number, but the fact that savage and civilised observers react differently to two illusions of a different kind suggests that we may possibly have in such comparative measurements a means of furnishing clues to the nature of an illusion. If an illusion is proved to be more marked to the savage, this would suggest that the illusion has a physiological basis, or at any rate that it depends on primitive and innate conditions the effect of which is masked by the results of experience in the more civilised. If, on the other hand, the illusion is less marked, the fact would suggest that the nature of the illusion is more complex and more dependent on the working of experience.

Tactile Discrimination.

The threshold for the discrimination of two points on the skin was tested by the method devised by Mr McDougall in Torres Straits. The important feature of this method is that the area of the skin which is being tested is touched with one point just as often as with two points. If stimulations with one point are only occasionally interspersed between the stimulations with two points, so that the latter are given more frequently, the results are almost certain to be biassed. If the observer either knows or thinks that he is being touched with two points more frequently than with one point, he will tend, in cases of doubt, to answer 'two' more often than 'one.' The error thus introduced can only be eliminated by an absolute equality in the number of single and double stimulations.

The compass points were applied at a distance from one another decidedly greater than the probable threshold, and the distance between them gradually diminished till the two points were no longer recognized as two. Twenty stimulations were made at each distance at which any error occurred, ten stimulations with one point and an equal number with two points. The distance taken as the threshold is that at which two mistakes in ten occur with each kind of stimulation.

In some cases, especially with European subjects, many mistakes with the single stimulations may be made when just above the threshold, and the standard which seemed to me most satisfactory was to allow two mistakes in ten with each kind of stimulation. A man who called two points 'one' twice and one point 'two' three times at a given distance would be rejected at that distance, and the distance next above it would be regarded as the threshold.

When the skin was touched with one point only, this was applied in the neighbourhood of one or other of the spots touched in the double stimulations.

Following the procedure adopted by Mr McDougall, the observer was told after each of his answers whether he had been right or wrong Mr McDougall adopted this procedure in order to make the experiment more interesting, and it is probably the most satisfactory method in the case of uninstructed people. For more educated subjects, it is doubtful whether the method is as satisfactory as that in which the observer is left in doubt as to the correctness of his answers, for the knowledge of the nature of a judgment often leads to speculation and the next answer is based, not on the immediate impression received when the skin is stimulated, but on processes of inference based on the degree of similarity of the second impression to the first. In other words, the method of telling the observer whether he is right or wrong often increases very greatly the part which inference plays in the process of sensory discrimination (*vide infra*, p. 366).

The areas of the skin tested were the middle of the flexor surface of the left forearm, the nape of the neck and the palmar surface of the terminal phalanx of the left index finger. In many cases the under surface of the second toe was also tested. On the forearm the stimulation was in a longitudinal direction; a point was marked midway between the elbow and the wrist, and the touches were applied at approximately equal distances from this point, so as to ensure that the same region of the forearm was tested. If this precaution is not taken variations in the threshold may be produced owing to the stimulations not being confined to the same region of the limb.

On the nape of the neck the points were applied transversely, and about equidistant from the middle line.

On the finger and toe the points were applied in a longitudinal direction.

Observations were made on 28 Toda men and 6 boys. In the following table I give the averages and other data of the 28 men, together with corresponding figures for 20 English men, Cambridge graduates and undergraduates.

TABLE XIV.

		Forearm mm.	Nape mm.	Finger mm.
Toda men, 28......	Mean	45·5	38·8	3·5
	Max.	60	70	8
	Med.	50	40	3
	Min.	25	15	2
	σ.	9·0	12·6	1·2
	C.V.	19·8	32·5	34·1
English men, 20 .	Mean	56·5	17·3	1·9
	Max.	70	30	3
	Med.	60	15	2
	Min.	40	10	1
	σ.	9·1	4·6	0·6
	C.V.	16·1	26·7	32·8

I also tested the under surface of the second toe. In three cases I failed to obtain a threshold within the limits of the toe. This may have been partly due to fatigue, but many of the men were also obviously embarrassed by this part of the test, and this almost certainly contributed to their failure. The average result for the other 25 men was 9·6 mm., but this was probably raised by the presence in many cases of the same causes which prevented the determination of a threshold in the other cases mentioned above. The highest threshold in this situation was 20 mm., and the lowest 3 mm.

I also tested six Toda boys whose forearms gave an average threshold of 35 mm., their napes 32·5 mm. and their fingers 2·2 mm., all smaller than those of the adults. For purposes of comparison I only have measurements from the forearms of 13 children of the village of Old Deer in Aberdeenshire, whose average threshold was 43 mm., smaller than the average threshold of the English adults, and bearing about the same ratio to that of the Toda boys as that existing between the adults of the two races.

The numbers taken are very small, but the results agree with the general experience that children have lower thresholds than adults. In order to see whether age had any influence among the Toda adults, I divided them into two groups. The average thresholds of the older group of 15 men over 35 years of age were 44·3 mm., 39·7 mm., and 3·9 mm.; those of the younger group of 13 men under 35 were 46·9 mm., 38·1 mm., and 3·0 mm. There is no appreciable difference in the acuity of the two groups.

The figures of Table XIV seem to show that the Todas are more sensitive than the English observers on the forearm, but much less sensitive on the nape of the neck and on the finger. The figures are, however, largely affected by various factors which have influenced the two groups of observers unequally. These factors are inference, practice, and fatigue, and there is little doubt that in their absence the figures of the table would have been very different.

The determination of the threshold for two points on the skin is greatly influenced by processes of inference. Many people answer "two," not only when they distinctly perceive that two points are touching the skin, but also when they receive the impression that a larger surface of the skin is being stimulated than in the single applications. They perceive no true doubleness but infer that two points are touching the skin on account of other characteristics of the sensation. It may happen that, at a given distance of the compass points, a person may answer correctly every time without having once clearly perceived two points, his answers being based on the discrimination of the difference in the nature of the sensations produced by the single and double stimulations.

In comparing two groups of individuals, it clearly becomes of the greatest importance to ascertain how far the answers of the two groups are based on the same criteria. If the Todas are only answering "two" when they distinctly appreciate two points, and if the English observers are giving the same answer when they infer that two points are being applied because of other differences in the character of the sensations involved, the figures of the two groups will not be directly comparable with one another. In the case of my English observers I know that their answers were largely based on inference, especially in the observations on the nape of the neck. Though I was not able to procure any introspective evidence on this point from the Todas, there are several features of the records which make it probable that the Todas were answering chiefly on the basis of the immediate sensory impression, while the English observers were largely depending, at any rate in their later observations, on indications of other kinds.

One means of distinguishing between the two methods of answering is to be found in the degree of abruptness of the change from the point at which every answer is correct to that at which many mistakes are made. An individual who is answering on the basis of the immediate sensory impression will at one distance be right every time; with a smaller distance he may have perhaps one or two wrong answers, and with the next smaller distance he fails entirely to distinguish two points from one. An individual, on the other hand, who is basing his answers on inferences will have two or three mistakes at one distance, and may then improve so that he passes the next stage or stages with fewer or no mistakes, and the transition to the point at which he entirely fails is gradual, so that it may in many cases be difficult to know exactly where to draw the line indicating the threshold.

I have compared the records of the Toda and English observers from this point of view. The subject is somewhat complicated by the fact that the kind of record which usually indicates that the answers are based largely on inference may also be the result of carelessness. A man who is not paying full attention will have occasional wrong answers even when distinctly above his threshold. In several such Toda records I noted at the time that the observer was careless, but even when such cases are included, the Todas show more cases of abrupt transition than the English observers; thus, of the 28 Toda men, 19 changed abruptly in the case of the forearm and 18 in the case of the nape, while of the 20 English observers, only half showed abrupt transition from above to below the threshold.

A second indication of difference in the procedure of the two groups of people is the great improvement of the English observers in the case of the nape as compared with the forearm so that their threshold, higher than that of the Todas on the forearm, became very distinctly lower on the neck. This improvement of the English observers was almost certainly due to practice in making use of sensory indications beyond the mere impression of doubleness. Several of them were fully aware of the fact that they were answering "two" not because they had two distinct sensations but because some sensations seemed more extensive than others, and they inferred that these were due to stimulation with two points.

The difference between the thresholds of the Toda and English observers on the nape of the neck is, however, very great, and we may be fairly confident that it is not wholly due to the factor just considered. It is probable that the high thresholds of the Todas in this situation and on the finger are partly due to fatigue or lack of attention owing to loss of interest[1]. In some cases this fatigue or *ennui* was fairly obvious and it probably accounts for the large standard deviation for the nape. In four cases the threshold for the nape came out higher than that for the forearm, and in these cases the average threshold for the finger was very high, viz. 5·0 mm., a result almost certainly due to defective attention.

If these suppositions are correct, the threshold of the Todas on the nape and index finger was raised by fatigue, while that of the English observers was lowered by practice in attention to subsidiary indications, and it is probable that there would have been no very great differences in the thresholds of the two people if these disturbing factors had not been present.

Another feature of the records which might be expected to assist in the elucidation of this point is the frequency of the answer "two" when only one point of the skin was touched. This error, which, in general, only occurs as the threshold is approached might be regarded as an indication that the answers are being based on inference or mere guessing rather than on direct observation. There is no doubt, however, that definite illusions of doubleness may occur when only one point of the skin is touched, and there is some reason to suppose that these illusions may have a physiological basis[2].

When a Toda said that two points were touching his skin when only one was being applied, it was impossible to tell whether he was experiencing a distinct or indistinct doubleness of the sensation or whether he was answering "two" because the sensation differed in quality, intensity, or extensity from that usually experienced when stimulated by one point. The significance of the error is therefore equivocal, but whatever the explanation may be, its frequency differed little in Toda and English observers. The error occurs especially as the threshold is approached, and it would be most satisfactory to determine the frequency of the error in the observations just above the threshold, but owing to the difference of the threshold in the two groups of observers, this method of comparison is not satisfactory, and I therefore give the number of errors in the total number of applications of the compasses. On the forearm the Todas called one point "two" 128 times in 1452 applications, or 8·8 °/₀, the English observers 88 times in 774 applications, or 11·4 °/₀. Here the English observers have the greater proportion; but on the nape the Todas called one "two" 104 times in 1131 applications, or 9·2 °/₀, and the English only 84 times in 1375 applications, or 6·1 °/₀.

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[1] In Torres Straits Mr McDougall found that when forearm, nape, finger, and toe were tested at one sitting, the process was too much for the patience of his subjects and in consequence he confined his attention to the two first situations.

[2] See Henri and Tawney, Wundt's *Phil. Stud.* 1895, Bd. xi. S. 394.

Taking the observations on forearm and nape together, there is little difference between Todas and English observers in the frequency of these illusory answers of doubleness. Since, however, the English observers had a smaller percentage of these answers in the observations on the nape than on the forearm, and since it is certain that inference played a much greater part in the former situation, it is probable that the frequency of the illusory answers is not due altogether to frequency of inference but is largely dependent on other causes.

We may, however, be confident that inference played a much greater part in the observations of the English than in those of the Todas, and though this applies especially to the observations on the nape, inference may also have played a part in the observations on the forearm, and if so, the superiority of the Todas in this situation would be more marked than the figures of Table XIV indicate.

The considerations I have brought forward show how great are the difficulties in the way of an accurate comparison of tactile acuity in different races, but the figures for the forearm suggest that the Toda is somewhat more sensitive than the Englishman.

The observations I have recorded in this section are in general agreement with those of Binet[1], who found in using compasses for the investigation of tactile sensibility, that people might be divided into three classes, "les simplistes, les distraits, les interprétateurs." There is little doubt that my English subjects belonged chiefly to the last group and the Todas chiefly to the first. In one respect the Todas differed from the 'simplistes' of Binet, who never gave the answer "two" when touched with one point only. It is possible that all the answers of this kind made by the Todas were due to distraction and that these people should be classed as 'distraits' rather than as 'simplistes.' My observations, however, differed in one important feature from those of Binet. The Todas had seen others being tested and therefore knew the general nature of my procedure, while Binet's subjects were not even allowed to see the instrument which was to be used. Binet has proved that 'simplistes' who are shown the instrument and are allowed to see for themselves that the points applied to the skin are like one another in every respect, may become at once 'interprétateurs.' The Todas probably interpreted their sensations to a certain extent owing to their acquaintance with the instrument and procedure employed, but their interpretations or inferences were probably of a simple character and played a smaller part than with the English subjects.

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[1] *L'Année psychologique*, 1903, IX. pp. 129—234.

TABLE XV. *Tactile Discrimination* (*compasses*).

Race	No.	Forearm mm.	Nape mm.	Finger mm.
Todas, Men	28	45·5	38·8	3·5
,, Boys	6	35·0	32·5	2·2
Murray Island, Men	50	19·8	11·6 (21)[1]	—
,, Boys (McDougall)	25	14·0	9·3 (18)	—
Dayaks (McDougall)	10	35·0	—	—
English Men	20	56·5	17·3	1·9
,, Children	13	48·0	—	—
English Men (McDougall)	23	44·6	20·8 (19)	—

In Table XV I give all the data at present available for the comparison of the tactile acuity of different races. The most striking feature of the table is the very low threshold of the Papuans of Murray Island as compared with other races. It is probable that the answers of the Murray Islanders depended chiefly on the immediate sensory impressions they received, though they may have been influenced to some extent by knowledge of the instrument which was being employed. Their low thresholds may be taken as indicating a degree of sensitiveness considerably greater than that of either English or Toda observers.

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[1] The figures in brackets give the numbers of individuals tested when they differ from those of the preceding column.

It will be noticed that there is a distinct difference between the figures of Mr McDougall and myself for English subjects, especially on the forearm. This may be partly due to a slight difference in the standard adopted for the threshold[1], partly to differences in the subjects. He tested inmates of a convalescent home, mostly belonging to the working classes, and he notes that five of those tested who belonged to the more educated classes gave a rather higher average threshold than the rest.

Taking the results for the forearm which are probably the most satisfactory for purposes of comparison, it will be seen that the Dayaks and Todas occupy a position lying between those of the Papuan and English subjects. In order of general intellectual and cultural development the Papuans would be at the bottom and the English at the top of the series, while Dayaks and Todas occupy an intermediate position. It is perhaps significant that the order should be the same when their tactile acuity is compared.

Tactile illusions. I tested many of the Todas to ascertain whether they were liable to the well-known illusion of Aristotle and its modifications, viz. that described by Croom Robertson[2], in which touches on the lower of two crossed fingers are localised in space above those on the upper finger and *vice versâ*; that described by Victor Henri[3], in which compass points close together one on each of the crossed fingers, are perceived as farther apart than compass points which are much more widely separated, and lastly, the modification described by myself[4] which is the converse of Aristotle's experiment, two objects applied to the outer sides of the crossed fingers being perceived as one object.

In educated Europeans these different forms of the illusion occur with different degrees of constancy and definiteness. Croom Robertson's form has perhaps the most universal success, followed by Henri's and Aristotle's, while the modification described by myself is least often successful on account of the difficulties attending the application of the stimuli. When I carried out these experiments with the Todas they succeeded perfectly in nearly every case. The four forms of the experiment succeeded perfectly definitely and at every trial. In one man Henri's form failed occasionally, the larger distance being recognized as the larger, and in another man the illusion failed in Aristotle's form and its converse.

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[1] Mr McDougall only used the stimulations with one point as a rough guide and did not take them into account in assigning the threshold.

[2] *Mind*, 1876, Vol. I. p. 145.

[3] *L'Année psychologique*, 1897, III. p. 225.

[4] *Mind*, 1894, N.S., Vol. III. p. 583.

In my experience these various illusions occur with the greater constancy and definiteness the more the observer is ignorant of the nature and aims of the experiments (the more without knowledge is the procedure), and it is noteworthy that in people in whom the procedure is wholly without knowledge, the experiments should succeed more perfectly and constantly than in any group of English subjects which I have tested.

Sensibility to pain.

The degree of sensitiveness to pain was tested with Cattell's algometer, an instrument by means of which graduated pressure can be exerted on an area of the skin and the pressure necessary to produce pain recorded in kilogrammes. I adopted the same general methods as were employed by Mr McDougall in Torres Straits. The instrument was pressed on the nails of the thumbs and forefingers, the subject of the test being told to call out "Stop!" as soon as he began to experience pain. Mr McDougall also tested the forehead, but I substituted for this situation the sternum, for I found I had to exert so much pressure with my instrument that I was afraid it might slip. I also tested the depression immediately above each knee-cap.

In English subjects it is found that the onset of pain is sharp and definite, and I had no doubt that the same was the case with the Todas. Observation showed that there was a rapid transition from a condition in which the people gave no sign of pain to one in which their expressions were very decided. In such a test as this there is an obvious danger that some individuals might regard the experiment as a test of the power of enduring pain, and might not speak till they had experienced pain for some time and could bear it no longer. Some support was given to the view that the Todas were looking on the test in this light by the fact that the algometer became one of the most popular of my instruments, and men would sometimes come to me especially in order to have their sensibility to pain tested. The onset of pain may, however, always be detected by means of slight involuntary flinching, and if the Todas had been treating the test as one of the power of endurance of pain, it would certainly have been detected by observation and I have not the slightest doubt that the Todas called out as soon as they felt pain.

The nails are very convenient places for the application of the algometer. In situations such as the sternum and forehead large variations often occur which are probably due to differences in the nature of the underlying parts, and the measurements are probably especially affected by the presence or absence of nerve-twigs in the area stimulated by the instrument. The advantage of the nails is that the pressure is evenly distributed over a given surface. It might be expected that differences in the thickness of the nails would influence the threshold, but I am very doubtful whether this is so. Some of the Todas had very delicate and thin nails, but the thresholds of these individuals were not lower than the average and I am doubtful whether differences of thickness, unless extreme, have any appreciable influence.

I was always careful to apply the instrument as nearly as possible in the centre of the nail, as different thresholds may be obtained if the instrument is applied near the base of the nail. The rate at which pressure is increased also influences the threshold. I did not attempt to employ any means of exact estimation, but I am fairly confident that the rate at which I apply the algometer is constant and that my observations on Toda and English observers are comparable in this respect. It is probable, however, that different experimenters apply the instrument at different rates and this would be a disturbing factor in the comparison of their results.

I tested 34 men, beginning with the right thumb and forefinger, then going on to the corresponding digits of the left hand. Then followed two observations on the sternum and one above each knee-cap. The observations in the last situation turned out to be unsatisfactory owing to the fact that the pain-threshold had not been reached at the upper limit of the instrument, viz. 15 kilogrammes. The results for the other situations were as follows:

TABLE XVI. 34 *Toda men.*

	Right Thumb	Right Forefinger	Left Thumb	Left Forefinger	Sternum 1.	Sternum 2.
Mean ...	9·7	7·7	8·9	7·4	8·9	9·3
σ.........	1·7	2·0	1·4	1·7	2·3	2·4
C.V.......	17·5	26·0	15·3	23·5	25·5	25·6

It will be noticed that the thresholds are higher on the right side than on the left, but as this might be due to the fact that the right side was stimulated first, I reversed the time-order in a further series of observations on 12 men, beginning with the left thumb. The thresholds were as follows:

TABLE XVII. 12 *Toda men.*

	Left Thumb	Left Forefinger	Right Thumb	Right Forefinger	Sternum 1.	Sternum 2.
Mean ...	9·1	6·8	9·0	7·3	8·2	9·0
σ.........	1·2	1·6	1·5	1·7	3·3	3·2
C.V.......	13·6	23·2	16·7	23·7	40·0	36·2

Here the two thumbs are almost exactly the same, but the comparison of the two sides is complicated by certain other features of the experiment. The first observation, made in this case on one or other of the thumbs, is always somewhat unsatisfactory. In beginning the test the subject is uncertain about the kind and nature of the pain he is about to experience[1]. Most commonly, when the pressure-sensations first produced by the instrument begin to be accompanied by pain, he is doubtful whether to speak or not, and often delays till the painful quality is obvious. Less commonly, so far as my experience goes, he is apprehensive and tends to call out before the pain really begins.

If the former attitude is the more common, it will raise the threshold for the first nail stimulated. If we assume that this was the case with the Todas, the right thumb should have had the higher threshold in the first series and the left thumb in the second series, and as a matter of fact this was so in the first series, while in the second series the two thresholds were approximately the same. The conclusion which is suggested is that the threshold on the right side is slightly higher than on the left side, and this conclusion is supported when we turn to the figures for the forefingers, which show in each series slightly higher thresholds on the right side. The difference is very slight, but, as we shall see later, exactly the same kind of difference is shown in the figures of the English observers.

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[1] In future observations it would be best to make an initial trial observation on some other nail in order to show the subject of the test what he is to expect.

On the sternum the threshold was somewhat larger on the second stimulation in each series and was much the same as on the thumbs.

I also tested three boys, three women, and two girls, who all had distinctly lower thresholds than the men.

TABLE XVIII.

	Left Thumb	Left Forefinger	Right Thumb	Right Forefinger	Sternum	
					1.	2.
Boys (8)	6·2	4·0	5·9	4·1	5·2	6·8
Women (3)...	5·9	4·6	6·1	5·8	—	—
Girls (2)	5·5	4·4	5·8	4·6	—	—

It is perhaps worth noticing that, except for the thumbs of the boys, the thresholds are higher on the right side. The thresholds are in every case decidedly lower than those of the men, and in the case of the boys it is probable that the difference is not due to any difference in the thickness or size of the nails, for the figures from the sternum bear about the same ratio to the figures of the men as in the case of the thumbs and fingers.

Age appears to have had no appreciable influence on the pain-thresholds of the adult Todas. Taking the 34 men who were tested on the right side before the left, the thresholds for the digits of a group of twelve men over 35 years came out 9·7, 8·7, 7·5, and 7·1, while the corresponding figures for 22 men under 35 were 9·7, 8·9, 7·9, and 7·6. The thresholds for the sternum are distinctly lower in the older group, being 8·3 and 8·7 as compared with 9·1 and 9·7. The coefficients of variation are, however, very much larger for the older than for the younger group.

For purposes of comparison I tested 40 English men, Cambridge graduates and undergraduates. In half I stimulated the right thumb and forefinger first; in the other half, the left side first. The following are the figures for the two groups:

TABLE XIX. *Right side first.*

	Right Thumb	Right Forefinger	Left Thumb	Left Forefinger	Sternum	
					1.	2.
Mean ...	6·0	5·2	5·8	5·1	5·1	4·9
σ.	1·3	1·1	1·1	1·2	1·6	1·1
C.V.	21·2	21·0	18·5	23·7	31·8	22·4

TABLE XX. *Left side first.*

	Left Thumb	Left Forefinger	Right Thumb	Right Forefinger
Mean ...	6·5	5·4	6·6	5·7
σ.	2·1	1·9	1·8	2·0
C.V.	31·9	36·0	26·7	35·8

In 20 cases I also took observations just above the knee-cap, one on each side, the average threshold being 9·5 kilogrammes. It will be remembered that the threshold of the Todas was certainly over 15 kilogrammes.

The thresholds are very distinctly lower throughout than those of the Todas. The ratio of the figures for the forefingers to those for the thumbs are approximately the same as for the Todas, and the same is true of the ratio between the figures for the sternum and digits. The difference between the two sides is very small, even smaller than for the Todas, but the figures are here consistently larger on the right side and suggest, though they cannot be said to prove, that in the English observers as with the Todas the right side is somewhat less sensitive to pain than the left side.

In the preceding tables I have given the coefficients of variation for each of the various situations tested. It will be seen that, in general, the coefficients are larger for the sternum than for the digits, a feature which might have been expected for the reasons already mentioned.

It will also be observed, however, that the coefficients are, on the whole, distinctly larger for the forefingers than for the thumbs and this is not, I think, what might have been expected. Owing to the complications which attend the determination of the threshold in the case of the digit first stimulated, I should have expected that one or other of the thumbs would have shown the greatest variability. In Tables XVI, XIX, and XX the thumb first stimulated has a larger coefficient of variation than the other thumb, but in all four tables the coefficients for the thumbs are smaller than for the forefingers, and often very decidedly smaller.

On comparing the coefficients of variation of the Todas and the English observers, it will be seen that the latter have distinctly the greater variability. The mean of the coefficients of the four digits for the Todas is 19·2, while that for the English observers is 26·8. There is nothing to tell us whether the larger coefficients of the English observers are due to differences in estimating the exact point of the onset of pain or to true variations in the degree of sensitiveness. The latter is perhaps here the more potent factor, and if so we have at least one measurement which shows greater variability in sensitiveness in the more civilised community. It is perhaps noteworthy that the coefficients for the sternum do not show the same relation between Todas and English observers, the figures differing little in Tables XVI and XIX, while in Table XVII the Todas have very large coefficients for this situation. Table XVII only contains the observations of 12 individuals and differences in the nature of the underlying parts may easily have influenced the threshold in some cases.

The apparent differences between the two sides raise an interesting question in connexion with the study of variability. The differences are very slight, either less than their probable errors or very little larger in all cases, and yet these slight differences occur in every one of the six groups tested. By ordinary statistical methods the differences of any one table would be neglected as below, or little more than, the probable error, but when the differences, though so slight, are found in every group tested, the question arises as to whether they do not indicate a real difference in sensitiveness.

The only observations on non-civilised races which are available for comparison are those made by Mr McDougall in Torres Straits and on a few Dayaks of Sarawak. In the Torres Straits *Reports* Mr McDougall gives also the thresholds of 23 English men. These differed so considerably from those given above that it seemed probable that there might be some difference between the instruments used in the two researches, although these had both been obtained from the same instrument maker. Mr McDougall kindly lent me his algometer, which was found to have a more rounded head than mine, so that the pressure of the instrument would be exerted on a smaller area of the nail or skin.

With this instrument I tested the nails of 40 individuals and it was found that pain was elicited with a distinctly smaller amount of pressure. I give the results in the following table because they illustrate some of the points already considered.

TABLE XXI.

	Right side first				Left side first			
	Right Thumb	Right Fore-finger	Left Thumb	Left Fore-finger	Left Thumb	Left Fore-finger	Right Thumb	Right Fore-finger
Mean ...	5·5	4·1	4·8	3·9	4·8	3·7	4·8	3·9
σ..........	1·5	1·1	1·3	1·2	1·8	0·8	1·3	0·8
C.V.......	26·8	28·0	27·4	29·4	37·2	22·1	27·2	19·7

The thresholds are distinctly lower throughout than with my own algometer, but they show higher thresholds on the right side except when the left thumb was stimulated first, the figures showing a very close correspondence in this respect with those of the second group of Todas. They add confirmatory evidence in favour of the suggestion that the right side has a lower degree of sensibility to pain than the left.

The observations with this instrument do not show the same difference in the degree of variability between thumbs and forefingers, and in the observations beginning on the left side the thumbs show by far the greater variability.

In order to compare the two algometers, I also made observations on the same individual on 24 days, using the two instruments on alternate days. The results were as follows:

	Right Thumb	Right Forefinger	Left Thumb	Left Forefinger
The flatter algometer ...	4·2	4·2	4·5	4·3
The rounder algometer.	3·4	3·3	3·6	3·3

Taking the values for all the digits together, the ratio between the figures of the two instruments is 1·26 : 1, while in the two groups of individuals given in Tables XIX, XX, and XXI, the ratio is 1·3 : 1.

In the following table I have summarised the results of my measurements for comparison with those obtained on Papuans and Dayaks by Mr. McDougall. The figures given for the thumbs and forefingers are the means of the figures of the two sides. The figures marked A are those obtained by the use of my own algometer; those marked B those of Mr McDougall's instrument.

TABLE XXII.

	No.	Thumbs	Forefingers	Sternum
Todas, A.	46	9·2	7·4	9·0
English men, A.	40	6·2	5·4	5·0
English men, B. (Rivers)	40	5·0	3·9	—
Murray Island men, B.	47	6·7	6·5	—
Murray Island boys, B.	18	3·8	3·3	—
Dayaks, B.	5	4·5	3·9	—
English men, B. (McDougall)	23	3·8	3·6	—

It will be noticed that there still remains a difference between the figures of Mr McDougall and myself with the B instrument, especially on the thumbs; the difference is not great and may be due to minor differences in the method of applying the instrument and in the rate at which the pressure is increased. Mr McDougall gave an initial trial observation so that his figures are free from the error which attaches to my observation for the thumb first tested. It will be noticed that it is in the figures for the thumbs that we differ most.

It does not follow that if the Todas had been tested with the B algometer, their figures would have borne the same ratio to those of the A instrument as in the case of the English observers, and the fairest procedure is to compare the ratio between the Todas and English men examined with the A instrument and the ratio between the Murray Islanders and English men tested by Mr McDougall. The former is in the case of the thumb 9·2 : 6·2 or 1·5 : 1; the latter is 6·7 : 3·8 or 1·8 : 1. For the forefingers, the ratios are 7·4 : 5·4 or 1·4 : 1 and 6·5 : 3·6 or 1·8 : 1 respectively. The ratios for the thumbs and forefingers agree in showing a greater difference between the Murray Islanders and English men than between the Todas and English men, and we may conclude that if all had been tested with the same instrument and with other identical conditions, the Todas would have occupied a position intermediate between the Murray Islanders and the English observers.

The ratios in the case of the Dayaks and English observers would be 1·2 to 1 for the thumbs and 1·1 for the forefingers. Thus, using the same standard of comparison, the Dayaks also come between the Papuan and English observers, but much nearer the latter.

In general intellectual development the Todas and Dayaks come between the Papuans and English men, and here, as in the case of tactile acuity, we see that they occupy the same position as regards sensitiveness to pain. Probably the Dayaks are superior in general intellectual development to the Todas, and if so, we find that four races arranged in order according to the degree of sensitiveness to pain are in their proper order according to the degree of intellectual development.

Smell.

In the determination of the olfactory acuity of the Todas I followed the example of Dr Myers in Torres Straits in using solutions of camphor. Camphor has many advantages as a test-substance. Its solutions lose strength very slowly, in my experience so slowly that there is no appreciable alteration during the time necessary for the observations. The substance has a definite and distinctive smell and for ethnological work there is the great advantage that this smell is familiar to natives of many parts of the world, owing to the wide use of camphor-wood boxes.

I used dilutions of a standard-solution[1] of the strength of one in a thousand, which I kept in a well-stoppered bottle, and the dilutions were presented to the natives in cylindrical glass tubes of exactly the same size and shape as those used by Dr Myers in Torres Straits.

In my earlier observations I followed the procedure used in Torres Straits, in which four tubes were presented, two containing water only, one a solution of camphor of a given strength and another of half its strength. I soon found, however, that it was very unsatisfactory to give solutions of two strengths together, the stronger solution influencing the perception of the weaker. Thus, when one man was given two solutions, of which one had a strength of 1/64000 and the other a strength of 1/128000, he detected the former on every occasion, but when this solution was presented with one of 1/32000 it was recognized only twice in ten presentations.

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[1] It is best to prepare a fresh standard-solution fairly often. At first I dissolved one gramme of camphor in a small quantity of alcohol and added 1000 c.c. of water, but the alcohol is not necessary, the camphor being completely soluble in that amount of water.

After several trials the procedure which I finally adopted was one, the principle of which is exactly the same as that of the test of tactile acuity with the compasses, tubes containing water representing the stimulations with one point. Four tubes were given, two containing a given strength of camphor solution while two contained water. Before beginning the test I prepared a series of solutions of the strengths 1/4000, 1/8000, 1/16000 and so on, down to 1/512000 or lower still. Fifty c.c. of a solution were poured into each tube, care being taken that the sides of the tube above the level of the solution should not be moistened. I did not cork the tubes because this might have led to contamination of the upper part of the tube, either with camphor or with some other odorous body. No native was allowed to touch a tube while smelling it, so that the solutions were not shaken.

By these means I attempted to ensure that the area of the surface of the solution giving off the odorous stimulus should be kept constant and limited to the cross-section of the tube, and also that no variations in the surface-tension of the liquid in the tube should lead to variations in the rate of liberation of the odorous stimulus.

Some time before beginning the observations I gave those who were to be tested a strong solution of camphor in order to ensure that they would know the smell in question.

I began the testing of each individual with a solution of which the strength was 1/64000. I placed on the table four tubes, two containing camphor solution of this strength and two containing water. The observer put his nose to each tube in succession and said, immediately after smelling each tube, whether he recognized camphor or not. The observer then looked away, the order of the tubes was changed and another series of judgments was obtained, and this was continued till ten camphor tubes and ten water tubes had been given.

I gave the tubes in one of the two following orders:

	W	C	W	C		C	W	C	W
	C	C	W	W		W	C	C	W
(A)	C	W	C	W	(B)	W	C	W	C
	W	W	C	C		C	W	W	C
	W	C	W	C		C	W	C	W.

If there was any possibility that an observer could have been influenced by knowledge of these arrangements, I adopted a third or fourth arrangement, and with European subjects I have not uncommonly done this, but there was never the slightest need for more than two in the case of the Todas.

These definite arrangements have one disadvantage. If above his threshold the subject of the test soon recognizes that two of the four tubes presented to him contain camphor and two water. Consequently, he tends to call the fourth tube "water" if he has already answered "camphor" twice, and "camphor" if he has answered "water" twice. It was evident that this was done occasionally both by the Todas and the English observers. This difficulty is easily overcome by presenting the tubes in groups of three, four, or five at a time.

A record was kept of the answers both with the water and the camphor tubes, and if the observer gave eight right answers out of the ten observations in each case, he was tested with a weaker solution. If he failed with the first solution, I passed on to a stronger. An interval of ten minutes at least was always allowed between a test with one strength and that with the next, and this was arranged by testing three or four individuals together, the others resting while one was being tested. The interval given was certainly long enough to allow any fatigue or adaptation produced by one series of observations to pass off before a new set of solutions was presented.

If an observer succeeded with the first solution 1/64000 and the second 1/128000, I went on to the third 1/256000 and so on, till I reached his threshold, and I usually persisted till I reached a point at which he had as many wrong as right answers and was obviously below his threshold. If he failed at the first strength, I gave 1/32000, and if he failed at this, 1/16000 and so on, but when he succeeded in detecting a given strength, I went on to the next weaker strength to see whether, with increased practice, he could now recognize an odour which he had previously failed to perceive, and I took the lowest strength which he succeeded in detecting eight times in ten trials as his threshold.

In determinations of a threshold, it is customary to begin either distinctly above the threshold and diminish the strength of the stimulus till the threshold is reached or distinctly below the threshold and proceed upwards—or to combine both procedures.

I departed from the usual custom, and adopted the expedient of beginning somewhere near the point which seemed from preliminary experiments to be the average threshold, for two reasons. The chief reason was to diminish as much as possible the number of observations and consequent fatigue or *ennui* which are such disturbing factors in threshold determinations. The expedient was more necessary in the case of smell than in testing other senses on account of the very large individual variations, for it would have been necessary to begin enormously above the threshold of the most acute if the initial stimulus had been strong enough to be above the threshold of the most obtuse. If I had begun with a strength of 1/1000, eleven sets of observations would have been necessary before reaching the threshold of the Toda who had the most acute smell and the observations would have lasted about three hours.

The other reason for beginning near the approximate threshold was that I might obtain in this way a large number of observations with a given strength of odour uninfluenced by the fatigue or practice induced by previous observations, and that the proportion of right and wrong answers obtained from the total number of individuals examined might serve as an index of the sensibility of one community as compared with another.

Beginning near the approximate threshold enables one to combine two comparative methods of studying sensibility; one, the actual determination of the threshold, the other the proportion of right and wrong answers with a given stimulus.

I tested 52 Toda men. The thresholds of three could not be determined satisfactorily. Of the remaining 49, one old man succeeded in smelling camphor only when the strength of the solution was 1/2000. A boy, Adikikudr, aged 14, only made one mistake with a strength of one in a million[1]. Between these two extremes there were wide variations; 3 succeeded at 1/8000, 4 at 1/16000, 11 at 1/32000, 14 at 1/64000, 10 at 1/128000, 5 at 1/256000. There was no case of complete anosmia. Age appeared to have a very marked influence on the sensibility: thus among 32 who were under 35 years of age, the threshold of 2 stood at 1/8000, of 1 at 1/16000, of 6 at 1/32000, of 8 at 1/64000, of 9 at 1/128000, of 5 at 1/256000, and of 1 at 1/1024000. On the other hand, among 17 men above 35 years of age, only 1 passed at 1/128000, 6 at 1/64000, 5 at 1/32000, 3 at 1/16000, and one each at 1/8000 and 1/2000.

I was able to test very few Toda women. Of four satisfactorily examined, 1 succeeded at 1/16000, 3 at 1/32000 and 1 at 1/256000. Four girls were on one occasion tested with the initial solution which none succeeded in detecting eight times in ten, but if I had been able to continue the series they might have improved. As I have already pointed out, the Toda women are such unsatisfactory subjects that it must not be concluded from these observations that the smell acuity of the women is lower than that of the men.

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[1] There are so many pitfalls in testing the acuity of smell that the result of this boy should be accepted with reserve.

I tested one Toda named Kòdrner on several occasions. He arranged for my regular supply of subjects and brought them to me every morning, and I tested him on eleven different days. His results from day to day were remarkably constant after the first occasion, when he only distinguished camphor 1/64000 five times in ten trials. On the second occasion he was right eight times in ten, and with the 1/128000 solution was right six times in ten, once also calling water camphor. After this he did not improve but gave much the same results every day, passing with the 1/64000 solution and failing with the weaker strength. He was subject to slight variations from day to day, and on one occasion made four mistakes with the stronger solution, while on another day he only made two mistakes with the weaker solution.

I also tested each of my interpreters on several occasions for comparison; one, a forest ranger, passed at 1/64000 on every occasion, and twice nearly passed at 1/128000. My other interpreter, a native of Tinnevelly, passed at 1/128000 on each occasion. Their thresholds corresponded with those most frequent among the Todas. One Englishman whom I tested in India under exactly the same conditions as the Toda made three mistakes in ten trials with the 1/128000 solution.

In the case of other senses we have found that the determination of the threshold for purposes of comparison of the sense-acuity in different races is complicated by the fact that inference may play a much greater part in determining judgment in one race than in another. This complication is perhaps greater in the case of smell than in any other sense.

If an English observer is asked to describe the smell he experiences as he proceeds from stronger to weaker solutions of camphor, it is almost always found that the smell-sensation changes in quality. At a strength of 1/64000, the smell is usually recognized as that of camphor. With the solution of 1/128000, the smell is still sometimes recognized as camphor; but with this and with weaker solutions it may become wholly different in quality, and the tubes containing camphor are probably distinguished from those containing water by means of this different smell, which is often so indefinite that observers are only able to say they smell something which they cannot describe.

This characteristic of threshold-determination, according to which solutions of an odorous substance give rise to indeterminate sensations at one strength and are only recognized as camphor at a higher strength, has been described by Toulouse[1], who, working with Vaschide, has made a large number of observations on the smell-acuity of different individuals, using camphor as the test-substance. Toulouse calls the lower threshold the "*minimum* of sensation" and the higher the "*minimum* of perception," and has found very great differences between the two. Thus in some observations on children by Toulouse and Vaschide[2] the minimum of sensation in one case (for boys 12 years of age) was at 1/1000000000 while the smell was only recognized as that of camphor at 1/5000. Similarly in my own observations on English subjects I have found wide differences between the point at which the smell of camphor is recognized as such and the point at which the camphor solution is recognized as different from water.

This ability to distinguish a solution of camphor when the smell of camphor cannot itself be recognized may depend on one or both of two causes. It may be that when camphor is present in very minute quantities, it produces an indeterminate sensation just as a very feeble coloured light can be seen although its colour cannot be recognized. On the other hand, it may be that when camphor is dissolved in water, some new substance is formed, different from camphor and having a different smell. If such a substance is formed, it would not necessarily follow that the amount of it present in the solutions would be directly proportional to the strength of the camphor solution.

That some such substance is formed seems to me not improbable from my own introspective observations and from those of some of my subjects. When I smell a very dilute solution of camphor, I sometimes experience a smell-sensation which has a definite, pleasant, aromatic quality, quite different from that of camphor.

The existence of two kinds of thresholds introduces great difficulties in the comparison of the degree of acuity of smell in different races. If the members of one race only distinguish camphor from water when they distinctly recognize the smell of camphor as such, while the members of another race distinguish the tubes containing camphor owing to the existence of some other smell, the two sets of observers might give the appearance of a great difference in sensitiveness while really having the same true threshold.

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[1] *C. R. de la Soc. de Biol.* 1899, Sér. XI. T. i. p. 879.

[2] *Ibid.* p. 487.

In my observations on English observers whom I have tested by exactly the same method as I used for the Todas, without paying attention to differences between the two kinds of threshold, I have found a degree of acuity superior to that of the Todas. The most frequent threshold was 1/64000, as was the case with the Todas, but many of my English observers detected much weaker solutions, while few failed with solutions stronger than 1/64000. The apparent inferiority of the Todas is probably, however, due to the difference above indicated. It is probable that the Todas, or at any rate the majority of them, only said a tube had the smell of camphor when they recognized the smell of camphor, while it is certain that the majority of my English observers said a tube contained camphor because they recognized that the solution differed in smell from water though they usually could not say in what the difference consisted.

There is another condition which interferes with any attempt at exact comparison of thresholds in Todas and Englishmen. Owing to the fact that it takes time to distinguish the altered criterion of discrimination, an English observer may often improve in his proportion of right answers for a time as he goes lower in the scale of strengths. With 1/64000 he may be right every time; with 1/128000 he may make three or four mistakes in ten, sometimes recognizing the smell as camphor and sometimes only noticing an indefinite smell. With 1/256000, he may make only one mistake and perhaps only fail definitely at one in a million.

If I had continued to test the Todas with weaker solutions after an apparent failure, it is possible that in some cases I might have obtained a lower threshold.

Owing to these difficulties, it seems to me useless to attempt any exact comparison between the Todas and my English subjects. My own determinations of the thresholds, taken uncritically, would show the Toda to be inferior in the acuity of smell to the Englishman, but this is certainly due in part, and perhaps altogether, to differences in the mode of making the judgments.

We have, however, another means of comparing the smell-acuity of the Toda and English observers. Every individual was first tested with a solution of the same strength, viz. 1/64000, and we can ascertain the proportion of right answers given by the two groups with this strength of solution.

For this purpose I am able to use the answers of the whole of the 52 Toda men who were tested. They gave 520 answers with the camphor solution and the same number with tubes containing water only. The camphor solution was recognized 283 times and was called water 237 times; the tubes containing water were correctly named 443 times; and were thought to contain camphor 77 times. With the camphor solution, the proportion of right answers is 54·4 °/₀; with the water tubes, 85·2 °/₀. The influence of age comes out by this method very strongly. For those below 35 years of age, the percentage of right answers with camphor is 63·9; for those above that age, 37·9; with water, below 35, 87·0; above 35, 82·1.

The English observers examined in the same way give distinctly higher percentages; with the camphor solution, there are 74·3 °/₀ of right answers; with water, 93·8 °/₀.

The English observers have a considerably larger percentage of right answers than the younger group of Todas with which they are most nearly comparable. These figures are derived from observations in which the factors which complicate the determinations of the threshold would come into play to a less extent than in the observations already described, and they point distinctly to the inferiority of the Todas to the English observers. Even here, it is probable that some of the English observers detected the tubes containing camphor, not because they recognized the smell of camphor, but because they detected some other difference between the two pairs of tubes. It is fairly certain, however, that this factor has played a much smaller part than in the actual determination of the threshold.

The comparative observations on the smell-acuity of the Todas and of English observers are perhaps chiefly important in the light they throw on the difficulties in the way of threshold-determination in the sense of smell. Owing to uncertainty about the degree to which inference may have played a part in determining the judgments, we cannot compare the results of the two sets of people exactly but we may be fairly confident that the Todas are not superior to English observers in the acuity of smell, and that they are probably somewhat inferior.

Travellers make frequent reference to the high degree of development of the sense of smell among non-civilised peoples, but very few attempts at exact observation have been made. In addition to my own observations and those of Dr Myers in Torres Straits, the only observations of which I know are those of Lombroso and Carrara on the Dinkas[1]. They found these people to have very obtuse smell when tested with essence of cloves, but their observations do not seem to have been very satisfactory. Both Dr Myers and I have found no striking difference between our uncivilised and our civilised subjects, the former being inclined to assign to the Papuan a somewhat higher degree of sensibility than was shown by his Scottish subjects, while I regard it as probable that the Todas fall somewhat short of my English observers in keenness of smell.

These observations have been made in very few cases, and only with one substance in each case, but they lend no support whatever to the idea that the acuity of smell is greater in savage than in civilised races. Any superiority of the savage in this as in other senses lies probably in his highly developed power of observation, in his practice in the discrimination and recognition of odours, and in his ability to assign a meaning to many of the smells he experiences.

Taste.

I limited my work on this sense to obtaining the names applied to the chief taste sensations. Even in doing this I had some difficulty, as the Todas refused to allow me to put the solutions into their mouths, and I had to let them dip their fingers into the solutions. The solutions used were of sugar, salt, dilute hydrochloric acid, and quinine. Some names for vinegar were also obtained. The sweet solution was called either *tütchiti* or *sakari* (sugar). The salt solution was called *ûp* (salt), one man only calling it *ûpûtütchiti* or sweet salt. The acid solution was called in general *pülchiti*, derived from the word for tamarind; one man called it *ûp*, and two men called vinegar *tein nîr* or honey water.

The bitter solution was called *kotchiti* by all.

There were thus definite names for the four pure taste sensations, the word for bitter being used even more unanimously than those for the other three sensations. The Toda language differs from many of the languages of primitive people[2] in having a definite word for bitter. The only decided confusion of two tastes with which I met in the Todas tested was that between salt and sour.

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[1] *Atti della Soc. Rom. di Antrop.* 1897, Vol. IV. p. 121 and *Archivio di Psichiatria*, 1896, XVII. p. 359.

[2] See this *Journal*, Vol. I. p. 124; and Torres Straits *Reports*, Vol. II. p. 186.

Hearing.

I made very few observations on the acuity of hearing owing to the difficulties in obtaining satisfactory conditions of experiment. The sounds which arise during the process of testing have so great an influence on the threshold that absolute measurements have little value, and the only satisfactory method is to compare the individuals directly. In our work in Torres Straits we compared the natives with one or more of the members of the expedition as standards. In the only observations I made with the Todas I used my Tinnevelly interpreter in the same way. I tested him in the open air together with a Toda, clicking the Politzer's Hörmesser, which I used as the source of sound, at different distances from the pair. Only two Todas were tested and both were distinctly inferior in auditory acuity to the Tamil. One man could only hear the sound at half a metre with the right ear, and at a quarter of a metre with the left, while the Tamil heard it at twelve metres. The same results were obtained in repetition of the experiment. The Toda in question was my guide, Kòdrner, who was very intelligent, and from my constant intercourse with him I had no reason to suspect that his hearing was less acute than that of his fellows. Another man heard the sound at 1·5 metres with one ear and ·5 m. with the other, the Tamil hearing it at the same distance as before. I should not be justified in founding any general conclusions on the hearing of two men, and further, my interpreter may have been a man of exceptionally acute hearing. I must be content with recording the observations.

Conclusions.

I may first consider how far the observations on the Todas lend any countenance to the belief that the acuity of the senses is higher in uncivilised than in civilised people.

The first point of interest is the very great difficulty which is introduced into the process of comparing sense-acuity in races of different culture by differences in the part played by inference in the process of determining the sensory threshold. In the case of the determination of visual acuity, this factor is reduced to very small dimensions by the E test, but it is not abolished. In the case of tactile acuity, its influence is very marked; it is possible for two individuals to give the same apparent threshold for two points and yet their real thresholds may be very different, for one individual may be distinctly discriminating the two points, while the other is only inferring from other characters of the sensory impression that two points are touching the skin. In the case of tactile discrimination, it is not, however, probable that these secondary factors will enable a person to answer correctly if very much below his real threshold.

It is when we come to the determination of the acuity of smell that this play of inference seems to reach its greatest scope. The more crude determination of the thresholds here seems to show the Toda is inferior to the English observer but it is probable that this is largely due to difference in the mental attitude in the two cases. Only one odorous substance was used and there seems to be little doubt that when this substance, camphor, is dissolved in water, some product is formed or liberated from combination which enables an individual to distinguish the camphor solution from water although he does not recognize the smell of camphor as such. Here again two people may have the same apparent threshold of sensibility for the smell of camphor; one may be smelling camphor and the other a substance altogether different. In the case of auditory acuity, it is probable that difficulties of this kind are less important, but their place is taken by equally great difficulties of another kind.

The effect of the play of inference in sensory discrimination is always, so far as I am aware, to lower the threshold, *i.e.* to give to an individual or group of individuals an appearance of sensibility which is higher than that they really possess.

On turning now to our actual results we find that in the case of visual acuity and the tactile acuity of the forearm, the Todas have a slightly higher degree of sensibility than more civilised observers. Any part which inference may have played would accentuate this difference, so that we are here justified in saying that in these two respects the Todas have greater sense-acuity than the English observers, but the difference is only slight. The great difference in the thresholds on the nape of the neck, on the other hand, is certainly largely due to the effects of inference in the one people and fatigue in the other, and as regards tactile acuity in general, we may say that the available evidence does not show any very important difference between the two peoples.

In the case of smell, we have a different state of things. Here, the crude determination of the threshold would show the Todas to be inferior to the English observers, but this inferiority may be apparent only, and due to the fact that the Todas probably only said a solution had the smell of camphor when they recognized it as camphor, while most Europeans distinguished the camphor solution by recognizing the presence of some secondary substance.

Still we may safely conclude from these observations that the Todas are in no way superior to Europeans in the sensibility for the smell of camphor and this seems also to have been the case with the Papuans of Murray Island tested by Dr Myers. Conclusions as to the acuity of smell based on tests with one substance only must be received with caution, but it is improbable that the Todas and Papuans would be more sensitive to other substances than they are to camphor, and the results with this substance do not in any measure corroborate the belief in the high degree of development of the sense of smell which travellers' tales might lead one to expect.

The available data on auditory acuity are still more scanty, but so far as they go they point to inferiority rather than to superiority in the Papuans and Todas.

The general conclusion which may be drawn from the available evidence is that pure sense-acuity is much the same in all races in the absence of definite pathological conditions such as errors of refraction and ruptured ear membranes, and that the frequent superiority of the savage over civilised man in his recognition of what is going on around him in nature is due to his trained powers of observation, powers usually limited in scope but very highly developed in special directions.

In the degree of sensibility to pain there is a decided difference between the Todas and English subjects, the former being distinctly the less sensitive. I have little doubt that the difference in their thresholds is due to a real difference in sensitiveness and not to any difference in the mode of making the observations. Thus somewhat greater sensitiveness of the skin in respect of tactile discrimination is associated with lower sensitiveness to pain. My English subjects were all of the educated classes and it is possible that the same difference of sensitiveness would not be found if the Todas were compared with such Europeans as agricultural labourers.

In two senses, age has had a marked influence on acuity. I have throughout divided the Todas examined into two groups, those above and those below 35 years of age. In visual acuity there is a falling off in the older group from 14·6 to 11·0, or nearly 25 °/₀. In smell the falling off is very great, being shown perhaps most clearly by the difference in the proportion of right answers with the 1/64000 solution of camphor which falls from 63·9% to 37·9%. In tactile discrimination, on the other hand, there is no marked difference. That the acuity should fall off in two cases and not in the third is a natural consequence of the fact that in the former cases the sense-organs are much more exposed to injurious influences: the tendency of the optic media of the eye to become opaque with age is undoubtedly assisted in the tropics by the more powerful action of light, while the olfactory membrane has to suffer from the constant irritation produced by the smoky huts in which the Todas live and from the effects of catarrhs to which they are liable in the cold season. There is no reason, on the other hand, why the sense-organs in the skin should suffer with age, and the thresholds of the older group in this test differ very little from those of the younger group. The difference in the different senses leaves little room for doubt that the falling off of acuity of sight and smell with advancing age is due to changes in the sense-organs and not to any change in the central processes involved in the tests in question.

On the sensibility to pain, age appears in the case of the Todas to have had little or no influence.

In the region of colour-vision, the most interesting feature of the Todas is the frequency of colour-blindness, far in excess of that which is known to exist in any other race. Those Todas who are not colour-blind exhibit exactly the same peculiarities of colour nomenclature which are so universally found in races of low culture, and associated with this is defective sensitiveness for blue, one of the colours for which their nomenclature is especially defective. The Todas add to the evidence in favour of the view that one of the factors upon which the defective colour nomenclature of the lower races depends is some degree of defect in sensitiveness.

In another sense in which the nomenclature of most races of the world is defective, that of taste, the vocabulary of the Todas is unusually complete, having definite terms for the four kinds of taste-sensation.

The material obtained in the investigation of illusions of different kinds confirms the results I have obtained elsewhere. One illusion, depending probably on physiological conditions, is experienced by the Todas more strongly than by the Europeans, while another, probably of a more purely psychological nature, is experienced less strongly, though only very slightly so. An interesting result of the observations on illusions is the definite evidence which is forthcoming that the processes involved in the tests are much simpler for the Toda than for the more civilised observer.

One of the features of such ethnological researches in psychology as are recorded in this article is that one is employing a procedure without knowledge (*unwissentliches Verfahren*) more completely than is perhaps ever possible in a civilised community, and in my observations on illusions, the effects of this are apparent. The results often come out more clearly and definitely in the case of the savage or barbarous community, and this is especially so with such observations as those on Aristotle's experiment and its modifications. The man of low culture reacts to such experiments with a less biassed mind; he attends simply to the matter to which he is asked to attend and does not speculate as to the why and wherefore of the experiments, and is consequently less distracted from the immediate task set to him.

The consequence of this is that in observations on the lower races results are obtained which are in general more definite and more consistent than those derived from more highly civilised individuals. The objection may be raised that this is due to the supposed high degree of suggestibility of the savage, but I am confident that this has not been the case either with the Murray Islanders or the Todas. The members of both these races showed a high degree of independence of judgment, and neither of them were easily influenced by suggestion, and I do not believe that races of their degree of culture are influenced in any great measure either by unconscious bias owing to the mental attitude of the experimenter or by consciously modifying their observations so as to give results which they believe will please him. I have little doubt that the latter procedure would make the observations of a group of observers inconsistent and would raise, not lower, the mean variations or standard deviations of the group. It is the former factor which is the more dangerous, but I do not believe that it played any appreciable part in my observations.

In connexion with the quantitative tests recorded in this paper I have given the coefficients of variability. The comparison of the variability of different races in their observations with psychological tests is a very complicated matter. The members of a group of individuals subjected to any such test may vary from one another in the nature of the physiological or psychological process which is the immediate subject of experiment. First—to take the case of an illusion—they may vary in the degree to which they are actually subject to the illusion.

Secondly, the members of the group may vary in the method of carrying out the test to which they are submitted. One individual may rely solely on the sensory data involved in the test, while another may bring a number of other factors into play to help him in making his measurements. To take the case of an illusion again; one individual may say that one line is equal to another when he sees them equal, while another may use various devices to help him to attain true equality, and again different members of a group may use different devices.

Thirdly, the members of a group may vary in the degree of care and attention which they give to their observations. *Ceteris paribus*, the more careful and attentive are the members of a group in any test, the smaller will be the coefficient of variation of the group. Lack of attention on the part of any member of the group will produce an aberrant measurement which will raise the coefficient of the whole group, and a group which contains many members whose attention is defective will show a high degree of variability. Similarly the coefficients of variation will be greatly influenced by the presence of aberrant measurements due to misunderstanding.

Lastly, different degrees in which fatigue and practice make their effects felt during a series of measurements will influence the variability. Two groups of individuals whose initial observations show the same degree of variability may yet differ greatly in the results of a prolonged series of observations, if in one of the groups some members improve by practice or fall off owing to fatigue in greater measure than their fellows.

In some cases it is not easy to tell which of these four sets of factors has been most potent in determining the differences in the coefficients of variation of the Todas and the English observers. In some cases, however, we can be fairly confident that one set has been largely responsible for the difference, though, of course, we cannot say how far other factors may also have been involved.

In the case of the illusions already considered, there can be little doubt that the coefficients of variation have been chiefly influenced by the second set of factors, and here we have seen that the same conditions may have very different effects on the coefficients of variation, according as the devices employed are successful or not in the attainment of true equality.

The larger variations of one English group, however, probably had a different cause. The coefficients of variation of the English children were very large, in one group enormously larger than in any other set of observers. We may be confident that this was not due to the complexity of their methods in performing the measurements, and I have no doubt that the large coefficients were due to the presence of children in the group who were either very careless or quite misunderstood the nature of the task which they were set. This carelessness or failure to understand is also shown by the largeness of the mean variations (m.v.) of their observations, which were larger than those of any other group tested (see Table X). The mean variations of the observations of the different individuals are very useful guides when one is endeavouring to ascertain whether the large coefficient of variation of a group of individuals is due to carelessness or misunderstanding.

In the observations on tactile discrimination the Todas varied from one another more than the English observers, especially on the nape of the neck, and this is almost certainly not due to the greater complexity of method in the case of the former, for I think there can be little doubt that owing to the greater part played by inference, the observations of the English observers were the more complex as regards method. The great variability of the observations on the nape were almost certainly due to fatigue on the part of some of the Todas, either acting directly in raising the threshold or leading to lack of the care and attention which the Todas usually devoted to their observations.

In the observations on sensibility to pain, the Todas had distinctly smaller coefficients of variation than the English observers, and though I have not given full data, the same would have come out in the case of the observations on acuity of smell. In the latter case, the high degree of variability was almost certainly due to greater variations in method, but I am doubtful as to the explanation of the smaller coefficients of the Todas in the observations on pain, and am inclined to believe that it indicates a lower degree of true variability in sensitiveness to pain.

In one other case it seems probable that high coefficients of variation show a high degree of variability of the process which was the special subject of the test, and that is in the observations with the tintometer. In the figures of Table I, the highest variability for red and yellow is shown by the Todas and English observers, while in the case of blue they are only surpassed by the natives of Egypt. The tintometer gives less scope for complexity of method than most of my other tests, and the existence of high coefficients of variation in the discrimination of colour side by side with great frequency of colour-blindness strongly suggests that in this case the coefficient of variation is largely a measure of true variability, probably of a physiological nature.

In several of the measurements for which Papuans, Todas and Englishmen have been tested, the figures for the Todas lie between those of the other two communities. This is the case with the observations for blue with the tintometer, with the observations by the older method of studying the illusions of compared horizontal and vertical lines, with the Müller-Lyer illusion, and with the mean variations (m.v.) of these measurements, and with the thresholds for tactile discrimination on the forearm.

In these observations the Todas occupy an intermediate position between the two other races and probably but for certain adverse circumstances[1], this intermediate position would have been occupied still more frequently. The interest of this lies in the fact that in general intellectual and cultural development, the Todas undoubtedly occupy a position intermediate between the Papuans and the Englishmen, and the corresponding intermediate position found in the above observations suggests that there is a connexion between general intellectual development and such mental operations as are tested by the experiments recorded in this paper.

The evidence at present is scanty but it certainly points towards the existence of some degree of correlation between general intellectual development and certain simple mental properties or activities which can be tested by experimental methods. The working out of any such correlation will, however, be a matter of the greatest difficulty. We have seen that the comparison between two races in a given measurement may be vitiated by slight differences in the nature of an instrument and they are certainly influenced by the inevitable slight differences of the procedure of different experimenters. The relation which seems to exist between the observations of Papuans, Todas and Englishmen has only come out as clearly as it has in this paper because most of the observations have been made with the same instruments, by the same methods and by the same experimenter in all three communities.

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[1] Thus if the observations with the algometer had been made with the same instrument and by the same observer, the results would probably have been intermediate. Further, the high threshold for red of the Todas was almost certainly connected with their great liability to colour-blindness. The observations on smell were conducted so differently on Todas and Papuans as to render comparison impossible.

A STUDY OF RHYTHM IN PRIMITIVE MUSIC[1].

By CHARLES S. MYERS,
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I. Peculiarities of rhythm in the music of the Malays of Sarawak. 1. *Description of their orchestra.* 2. *The method experimentally employed to record the rhythm.* 3. *Analysis of the records.*

II. Further evidence of rhythmical complications in primitive music. 1. *Analogy of the above records with the rhythm of early Indian music and with Greek metre.* 2. *The use of simultaneous contrary rhythms.* 3. *The use of frequently changing rhythms.*

III. The place of rhythm in primitive and advanced music.

I.

1. During a visit to Borneo I had the opportunity of investigating some exceptionally complicated methods of gong-beating which are in vogue among the Sarawak Malays.

Their orchestra usually consists of (1) a set of seven or eight small gongs or *kranang* of different pitch, which are arranged before the player in horizontal series upon a frame of bamboo and rattan, (2) a large gong or *chanang*, (3) two small drums or *gendang*, and (4) a still larger gong, the *tawak* or *tetáwak*. The *kranang*, *chanang* and *gendang* always keep excellent time with one another. The gongs of the *kranang* are rapidly sounded, about six per second. The *chanang* emits a high note and regularly accentuates the first of every four sounds of the *kranang*. The two *gendang* are called 'mother' and 'child.' When one of them is silent, the other is beaten; together, their rhythm supports that of the *kranang*, the accented beat coinciding with that of the *chanang*.

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[1] A paper communicated to the Psychological Society on April 1st, 1905.

On the other hand, the *tawak*, a large gong of about sixty centimetres in diameter, is played in a rhythm seemingly quite independent of that of the other instruments of the orchestra. It may be beaten in various modes, but they are alike characterized by a complete absence of time, so far as can be appreciated by the European ear. The accented beats appear to recur with incomprehensible irregularity. I remember that on one occasion the player of the *tawak* becoming tired, he passed on the instrument to another Malay who proceeded to beat it just as a European would do, keeping strict time with the orchestra. He was laughed at by his audience and very soon retired covered with ridicule. It was evident that only an expert could play it.

No movement of the lips or feet accompanies this curious rhythm. It is listened to without singing or dancing. The unaccented beats of the *tawak* are damped by the fingers of the player's left hand. His right hand at one time beats the body, at another the raised central boss of the instrument. The *tawak* is also used by the Kenyahs and other peoples of Sarawak to carry news of death, war, childbirth, etc., to neighbouring villages, each mode of beating having a recognized meaning[1].

2. I endeavoured to investigate the peculiar methods of sounding the *tawak* in the above orchestra by allowing a Malay to tap upon a Morse key just as if he were beating the *tawak*, while the other instruments were being played as usual. The taps thus made were recorded upon the travelling surface of a smoked drum by means of an electrically driven time-signal. Below the time-signal a clock recorded intervals of a fifth of a second on the smoked surface. In this way I was able to obtain a record of the number of tenths of seconds which elapsed between successive beats of the *tawak*. I hoped that a leisured visual study of these intervals would lead to an understanding of this curious performance which to the European ear appeared so completely devoid of system, defying every attempt at rhythmization.

3. The Malay who thus recorded the sounds of the *tawak* told me that there were many methods of beating it. I have records of four different methods and I present them here. Unfortunately my attention was confined too exclusively to the recording apparatus to allow me to take note of the position of the accented beats[2] or to observe if they ever coincided with those of the other instruments. My figures merely give the intervals in tenths of seconds which elapsed between successive

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[1] There is no reason to believe that the distribution of beats and accents corresponds with the number and position of accented and unaccented syllables of words understood.

[2] The accented beats of the *tawak* are naturally longer, as a rule, than the unaccented beats.

tawak-beats. But I am convinced by my experience on other occasions that the rhythm of the latter was completely independent of that of the rest of the orchestra.

METHOD I.[1]

$$\overbrace{\overset{-}{3{\cdot}5},\ 3,\ 1{\cdot}5}^{d},\quad \overbrace{2,\ 3}^{},\quad \overbrace{\overset{-}{3{\cdot}5},\ 3,\ 1{\cdot}5}^{d},\quad \overbrace{2,\ 3}^{},\quad \overset{+}{5{\cdot}5},\ 2,\ 3{\cdot}5,\ 1{\cdot}5,\ 3{\cdot}5,$$

$$2,\ 2{\cdot}5,\ 1{\cdot}5,\ 3{\cdot}5,\ 4,\ \overset{+}{2{\cdot}5},\ 2,\ 3,\ 2{\cdot}5,\ 2,\ \overset{+}{2{\cdot}5},\ 1{\cdot}5,\ \overset{+}{2},\ 3,\ \overset{+}{2{\cdot}5},$$

$$2{\cdot}5,\ 1{\cdot}5,\ 2,\ \overset{+}{2{\cdot}5},\ \overset{-}{3{\cdot}5},\ 3,\ 2,\ \overset{+}{2},\ 2{\cdot}5,\ 3{\cdot}5,\ldots\ldots\overset{+}{2{\cdot}5},\ \overset{-}{3{\cdot}5},\ \overset{+}{2{\cdot}5},\ 1{\cdot}5,$$

$$2,\ 3,\ 3,\ 1{\cdot}5,\ 2,\ldots\ldots\overset{+}{2},\ 1{\cdot}5,\ \overset{+}{2{\cdot}5},\ 2,\ \overset{+}{2{\cdot}5},\ 1{\cdot}5,\ 1{\cdot}5,\ \overset{+}{2{\cdot}5},$$

$$*\overbrace{3{\cdot}5,\ 4}^{a},\quad \overbrace{2,\ \overset{-}{3{\cdot}5}}^{},\quad \overbrace{3,\ 4{\cdot}5}^{b},\quad \overbrace{2,\ \overset{+}{2{\cdot}5}}^{},\quad \overbrace{3,\ 1{\cdot}5,\ 3}^{c},\quad \overbrace{2,\ 3}^{},\quad \overbrace{\overset{-}{3{\cdot}5},\ \overset{+}{4}}^{a},$$

$$\overbrace{2,\ 2{\cdot}5}^{},\quad \overbrace{\overset{-}{3{\cdot}5},\ 3{\cdot}5,\ 1{\cdot}5}^{d},\quad \overbrace{2,\ 2{\cdot}5}^{},\quad \overbrace{\overset{-}{3{\cdot}5},\ 4}^{a},\quad \overbrace{2,\ 3}^{},\quad \overbrace{\overset{+}{2{\cdot}5},\ 3{\cdot}5,\ 1{\cdot}5}^{d},\quad \overbrace{2,\ \overset{+}{2{\cdot}5}}^{},$$

$$\overbrace{\overset{-}{3{\cdot}5},\ 1{\cdot}5,\ 2{\cdot}5}^{c},\quad \overbrace{2,\ 3}^{},\quad \overbrace{3{\cdot}5,\ 4{\cdot}5}^{a\text{ or }b},\quad \overbrace{2,\ 3}^{},\quad \overbrace{3,\ \overset{+}{2{\cdot}5},\ 1{\cdot}5}^{d},\quad \overbrace{2,\ 3}^{},$$

$$\overbrace{3,\ 4{\cdot}5}^{b},\quad \overbrace{2,\ 3}^{},\quad \overbrace{3{\cdot}5?,\ \ldots\ 1{\cdot}5}^{d},\quad \overbrace{2,\ 3}^{},*\ 4,\ 4,\ 2,\ \overset{+}{2},\ 3,\ 4,\ 2.$$

I was for a long time puzzled at the apparent lack of regularity in these figures. But I succeeded at last in systematizing that section which lies between the two asterisks. There can be no doubt that here the beats are grouped in two alternately recurring 'bars' which are of different lengths. The one comprises 7·5 units (i.e. tenths of seconds), the other 5 units. The beats within the latter bar are always two in number and always have the values 2 and 3. Those within the former bar may be two or three in number, with the values (*a*) 3·5 and 4, (*b*) 3 and 4·5, (*c*) 3, 1·5 and 3 or (*d*) 3, 3 and 1·5. The figures sometimes do not tally precisely with this plan, but the absolute value of such deviations is so small as to be negligible. In other words, then, if we represent one unit interval by the quaver[2], the various groups met with may be written thus:

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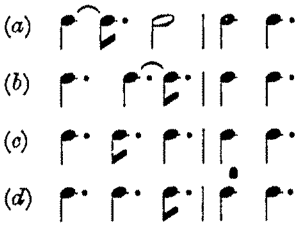
[1] Each figure represents the number of tenths of seconds between successive beats. The signs + or − over a figure indicate that its value is very slightly higher or lower than that recorded. A dotted line shows where the record is undecipherable. The brackets and letters are explained later.

[2] The metronome-rate thus being, 𝅗𝅥 =150.



An alternation of such measures would of course fail to be appreciated by the European ear.

METHOD II.

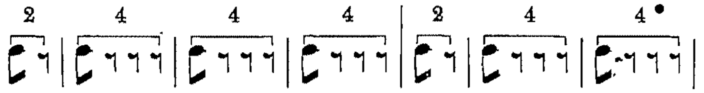
$$\ldots\ 3,\ 4,\ \overset{+}{4},\ 6,\ \overbrace{2,\ \overset{-}{4},\ 4,\ 4,\ 2,\ 4,\ \overset{+}{4},}^{a}\ 2,\ \overbrace{3,\ 5,\ 2,\ 6,\ \overset{+}{2},\ 2,\ \overset{+}{4},}^{b}$$

$$\overbrace{\overset{+}{2},\ \overset{-}{4},\ 4,\ \overset{+}{4},\ 3,\ \overset{-}{4},\ 4,}^{a}\ 5,\ \overset{+}{2},\ \overbrace{3,\ 5,\ 2,\ 6,\ \overset{+}{3},\ \overset{+}{2},\ 4,}^{b}\ 4,\ \overbrace{3,\ 4,\ 3,\ 3,\ 2,\ 3,}^{d}$$

$$6,\ \overbrace{\overset{+}{2},\ 4,\ 3,\ 1,\ \overset{+}{2},\ \overset{+}{2},\ 4,\ 3,\ 2,\ 2,}^{c}\ \overbrace{3,\ 4,\ 4,\ 3,\ \overset{+}{2},\ 4,\ [illegible],}^{e}\ \ldots$$

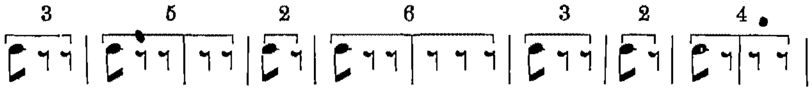
Here again we have evidence of the grouping together of measures of various duration so as to form longer periods (*a*) and (*b*) which are subsequently repeated. These periods or groups may be separated by one or more beats from one another.

Group (*a*) which is repeated may be represented thus:

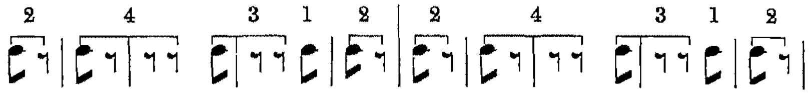


There is a change from a two-pulse to a four-pulse period.

Group (*b*) which is also repeated may perhaps be represented:



Group (*c*) contains the double group 24312. It may be written:



Alike in (*b*) and (*c*) we have alternating groups of two and three-pulse periods,—a condition closely analogous to that subsequently described in American-Indian music (p. 404) and in Greek metre (p. 402).

The sum of the intervals within group (*c*) is approximately twenty-five units: this is likewise the value of each of the groups (*a*) and (*b*). Possibly groups (*d*) and (*e*) are also equivalent.

METHOD III.

$$\overset{-}{4},\ \overset{+}{4},\ \overset{+}{2},\ \overset{+}{2},\ 4,\ 6,\ 1,\ \overset{-}{4},\ 8,\ \overset{+}{4},\ 2,\ 8,\ 3,\ \overbrace{\overset{-}{4},\ \overset{-}{4},\ 3,\ 4,\ 2,}^{a}\ 4,$$

$$\overbrace{\overset{-}{2},\ 3,\ 2,\ 8,\ \overset{-}{2},\ 2,\ \overset{-}{4},\ \overset{-}{4},}^{b}\ 8,\ \overset{-}{2},\ 2,\ 3,\ \overset{-}{4},\ 4,\ 2,\ \overbrace{\overset{-}{4},\ 4,\ 3,\ \overset{-}{4},\ \overset{-}{2},}^{a}$$

$$2,\ 3,\ \overbrace{2,\ \overset{+}{2},\ 2,\ 8,\ \overset{-}{2},\ 2,\ 3,\ \overset{-}{4},}^{b}\ \overbrace{5,\ 4,\ 3,\ 5,\ 2,}^{c}\ \overset{-}{4},\ 4,\ \overset{-}{2},\ \overset{-}{2},$$

$$\overbrace{5,\ 8,\ \overset{-}{4},\ 5,\ 2,}^{c}\ 3,\ \overset{-}{4},\ 4,\ 2,\ \overset{-}{4},\ 3,\ 5,\ 2,\ 2,\ \ldots$$

METHOD IV.

$$\ldots\ \overbrace{\overset{+}{4},\ 4,}\ 2,\ 2,\ \overbrace{2,\ 4,\ 2,\ 1,}^{a}\ \overset{-}{2},\ 2,\ \overbrace{1,\ 4,\ 2,\ 1,\ 2,\ \overset{-}{1},}^{b}\ \ldots\ \overbrace{2,\ 4,\ 2,\ 1,}^{a}$$

$$\overbrace{4,\ 4,\ \overset{-}{4},}^{c}\ 1,\ 2,\ \overbrace{1,\ \overset{-}{4},\ \overset{-}{4},}^{d}\ \overbrace{1,\ \overset{-}{4},\ \overset{-}{4},}^{d}\ \overbrace{1,\ \overset{-}{4},\ \overset{+}{2},\ 1,\ 2,\ 1,}^{b}\ 2,\ \overset{+}{1},$$

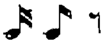
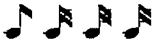
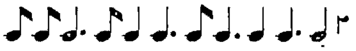
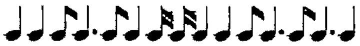
$$\overbrace{2,\ 4,\ 1,\ 2,}^{e}\ \overbrace{4,\ 4,\ \overset{-}{4},}^{c}\ \overbrace{2,\ 4,\ \overset{+}{1},\ 2,}^{e}\ 4,\ 1,\ 4,\ \overset{+}{2},\ 8,\ \overset{+}{4},\ 6,\ 8,\ 8,\ 6,\ \overset{-}{6}$$

In these two methods, likewise, we find that beats, separated by intervals of different lengths, are gathered together into distinct groups, each group being divided by one or more beats from neighbouring groups. The third method closes with a succession of very rapid beats, the fourth with a succession of very slow beats.

In analysing the above records, it must be remembered that the performer was placed at a serious disadvantage while they were being made. There can be no doubt that he was disconcerted by the substitution of a Morse key for his accustomed instrument. To the consequent inaccuracies in his performance I am inclined to attribute in great part our inability to recognize methodical grouping in certain portions (especially at the beginning) of each record.

II.

1. From a study of these records it is clear that the Malays of Sarawak are able to regard many successively different intervals of time as a coordinated whole which they recognize when repeated in the course of the performance. This faculty they carry to a degree which lies so far beyond the powers of civilized musicians, that the latter may reasonably be sceptical as to the possibility of its occurrence among less advanced peoples. I have, however, been fortunate in finding excellent corroborative evidence in ancient India. Day[1] gives a table of some forty rhythmical periods or *tâlas* of early Indian Music. While their number and definition vary according to different authorities, each *tâla* had its own name and its mark of notation. I select four from Day's list:

Name	Value in English notation	Length of successive beats, 𝅘𝅥𝅯 =1
Tritéya		1, 4
Simhalîla		2, 1, 1, 1
Simhavikrîdita		2, 2, 6, 2, 4, 6, 2, 6, 4, 6, 8
Simhânandanâ		4, 4, 2, 6, 2, 4, 1, 1, 4, 4, 2, 6, 2, 6, 4

Day found it difficult to believe that such complex *tâlas* were ever "in very common use." But the analyses which we have just made of the methods of beating the Bornean *tawak*, leave no room for doubting that the early Indians, like the Malays, enjoyed the faculty of combining successive dissimilar periods and of regarding them as members of a complex unity.

The rhythms of the Ancient Greeks and of the Arabs[2] were scarcely less complex. The fourth verse of the Greek alcaic metre, for example, ran thus:

– ⏑ ⏑ | – ⏑ ⏑ | – ⏑ | – ⏑ | .

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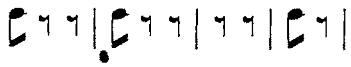
[1] Captain C. R. Day, *The Music and Musical Instruments of Southern India and the Deccan*, London, 1891, p. 26.

[2] R. G. Kiesewetter, *Musik der Araber*, Leipzig, 1842, S. 49—57.

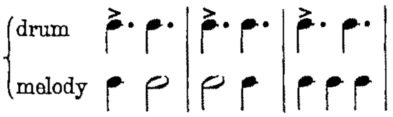
We may compare with it our interpretation (p. 400) of part of group (*b*) in the Malay method II.:



The pæonic and hemiolic rhythms of the Greeks are also remarkable[1], founded as they were on the ratio 3 : 2. Each of the five beats, according to some authorities, could be subdivided into five so that the whole foot might contain the ratios 15 : 10. It will be remembered that this is the precise ratio of the two parts (7·5 : 5) which repeatedly recur in method I. of the *tawak*-beating.

Doubtless these poetic metres were overlain by musical rhythms, just as in ruder form the beats of the Malay *tawak* accompanied the gong and drum orchestra. According to Headlam[2] many of the complexities of Greek lyric metre may be attributed to an overlapping of rhythms *one by another*. He attributes their æsthetic value to an effect resembling counterpoint in music.

2. The complexity of the rhythms which may be employed by primitive peoples has been already noticed by some students of their music. Thus, Fillmore[3] wrote of the Omahas, an American Indian people: "I know of no greater rhythmical difficulties anywhere in our modern music than these Omahas have completely at command in their every day music.......Rhythm is by far the most elaborately developed element of the Indian music, and in this respect civilized music has not surpassed it, at least in the point of combining dissimilar rhythms" (pp. 67—68). Their Hae-thu-ska dances, for example, "require the double drum beat, a strong pulse followed by a weak one. Against this many of the songs have three equal notes or their value" (p. 67). Thus the Song No. 19 (p. 93) contains the following bars:



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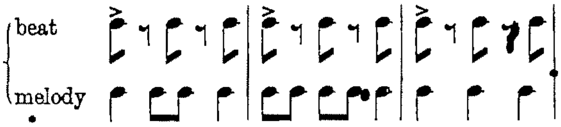
[1] A. W. Ambros, *Geschichte der Musik*, Leipzig, 1880, Bd. I. S. 432.

[2] *Journ. of Hellenic Studies*, London, 1902, Vol. XXIII. pp. 216, 219, 225.

[3] "A study of Omaha Indian Music," by Alice C. Fletcher, Francis La Flesche, and John Comfort Fillmore, *Archæological and Ethnological Papers of the Peabody Museum*, Harvard Univ., Cambridge, Mass., U.S.A., June, 1893.

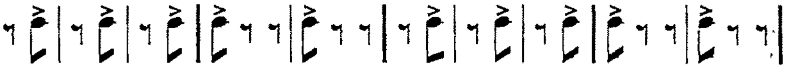
Franz Boas[1] transcribes (pp. 673, 687, 703) several songs of the Kwakiutl Indians of this type:



He remarks that "the beating is perfectly regular, five-eighths against three [fourths] of the tune" (p. 673). In other songs of the same collection different rhythms are opposed in a still more remarkable fashion.

Dittrich[2] observes that "the feeling for rhythm is very strongly developed among the Japanese. The most difficult syncopations...are performed with a precision that would astonish even a genuine musician in Europe" (p. 387). Stumpf[3] also alludes to syncopation in Siamese, and Land[4] in Javanese music.

3. Frequent change of rhythm has been likewise noted as a characteristic of certain primitive music. Thus, according to Abraham and von Hornbostel[5], several Turkish songs contain bars of three beats interposed between bars of four beats. In their study of the music of India, the same observers[6] write: "we find syncopation and change of rhythm so frequent that we are often unable to detect any constant primary rhythm at all, but are compelled to imagine a continual modification of measure" (p. 398). For example, Song No. 14 contains bars of 2, 3, 4, 6, 8 and 9 crochets; Song No. 18 contains bars of 8, 9, 10 and 12 quavers. In Song No. 20 the drum-beats show regularly alternating groups of two-pulse and three-pulse periods:


etc.,

while the melody runs its course independently of this contrary rhythm, which forms a "kind of rhythmical counterpoint" overlying it.

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[1] "The Kwakiutl Indians," Smithsonian *Reports*. U.S. National Museum Reports for 1895, Washington, 1897.

[2] "Beiträge zur Kenntniss der japanischen Musik," *Mittheil. d. deutsch. Gesell. f. Natur- und Völkerkunde Ostasiens in Tokio*, Tokio, 58ᵗᵉˢ Heft, Jan. 1897.

[3] "Tonsystem und Musik der Siamesen," *Beitr. z. Akustik u. Musikwissenschaft*, Leipzig, 1901, Bd. I. Heft 3, S. 111.

[4] "Die Tonkunst der Javanen," *Vierteljahrsschr. f. Musikwissenschaft*, 1889, Bd. v.

[5] "Phonographirte türkische Melodien," *Zeitsch. f. Ethnologie*, Berlin, 1904, Bd. XXXVI. S. 203—221.

[6] "Phonographirte indische Melodien," *Sammelbände d. internat. Musik-Gesell.*, Leipzig, 1904, Jahrg. v. S. 348—401.

Similar evidence is mentioned by some earlier writers upon the subject, who, moreover, emphasize the ability of the American Indians[1] and Indians[2] to perform five- and seven-pulse measures.

Describing the modern music of India, Day writes (*loc. cit.* p. 36), "The different degrees of time are called Tâlas, of which there are seven, each being subdivided into five 'jâtis' or kinds; so that there are in use no less than thirty-five distinct measures. By the annexed table the various tâlas and their respective jâtis will be understood at a glance, the figures signifying the number of beats of equal duration made in a bar."

Name of Tâla	Name of Jâti				
	Chaturúshra	Tishra	Mishra	Cúndha	Sankírna
Dhruva	4244	3233	7277	5255	9299
Mâtsya	424	323	727	525	929
Rupaka	42	32	72	52	92
Jhampa	412	312	712	512	912
Triputa	422	322	722	522	922
Atatâla	4422	3322	7722	5522	9922
Ekatâla	4	3	7	5	9

III.

The rhythmical characteristics of the examples of primitive music which we have studied are three in number: a delight in change and in opposition of rhythm, and a demand that relatively long periods filled with measures of diverse length be apprehended as an organic whole or 'phrase.' To such an extreme have we found these features occasionally to be carried that their æsthetic effects may be neither appreciated nor reproducible by more advanced peoples.

Indeed among ourselves the progress of music has been by the elaboration of harmony rather than of rhythm. But just as the complexities of European harmony have developed from a basis of simple relation between the vibration-frequencies of simultaneously occurring tones, so those of primitive rhythm have developed from a basis of simple relation between the duration of successively occurring periods. The development of harmony and of rhythm alike invoke the psychological acts of analysis, synthesis and fusion.

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[1] "Lieder der Bellakula Indianer," von C. Stumpf, *Vierteljahrsschr. f. Musikwissenschaft*, 1886, Bd. II. S. 409.

[2] *Hindu Music from various Authors*, compiled by [Rajah Sir] Sourindro Mohun Tagore, Calcutta, 1875, pp. 51, 261.

While the later advances in choral singing in Europe required a more regular and a more frequent accent than was necessary in earlier stages of European culture, primitive music, unhampered by the demands of harmony and polyphony, has evolved complications of succession rather than of simultaneity,—complications of measure rather than of tone.

In the early mediæval music and among the existing folk-songs of many parts of Europe, curious irregularities or even defects of rhythm are met with. Within relatively modern times our composers have successfully obtained novel and striking effects by departures from the uniform and more conventional rhythms. Whether they will ever adopt such complex rhythms as are in use among certain primitive peoples, must depend on the gradual education of their audience and on the limiting value of the strain of attention which is compatible with æsthetic pleasure.

IS 'BLACK' A SENSATION?

BY JAMES WARD.

The majority of experts answer in the affirmative, but the plain man, when the question is understood, in the negative.

The latter position briefly stated: darkness and blackness distinguished as negative sensation and positive perception.

Examination of the reasons advanced for the former position:—

There is no exact parallel between darkness and stillness; but only because blackness has become a 'secondary quality' of objects. The 'dark-field' only extended when interpreted as visually empty space. Helmholtz on the whole favours the negative character of black.

Theories to explain black on the assumption of its positive character:—

Hering's theory starts from the facts of 'contrast,' but these not incompatible with the negative character of black; implies a 'blinding black'; inconsistencies in his treatment of achromatic and chromatic antagonism; his medium grey at variance with the facts of visual adaptation; cannot be identified with the darkness of the adapted eye as his theory requires; the assimilative process said to underlie black not proven.

Wundt's eclectic expositions: black as the limit of an intensive series; black as positive and the qualitative opposite of white, first constant in intensity and then variable; black as an anabolic, inhibitory process which is yet an independent one.

G. E. Müller's theory: an attempt to remedy the defects of Hering's; accounts for the series of greys by variations in an endogenous 'medium grey' which result from retinal excitations; this assumption seemingly insufficient to meet the facts; its physiological improbability; its 'singularity' not really explained.

Conclusion: desirable to let the facts speak for themselves; the older and simpler view not yet exploded. Hering has corrected Helmholtz but has not yet established his own theory: its difficulties awaken the suspicion that the assumption of the positive character of black as a sensation is false.

To this question an affirmative answer would be given as a matter of course by everybody unversed in psychological enquiries, and this answer most psychologists and physiologists nowadays seek to confirm by more or less elaborate arguments. Such unanimity seems to constitute a strong presumption of truth. But if any layman were asked whether he has any visual sensation in the dark he would equally without hesitation answer in the negative, and would explain that when he talks of seeing black he means what in psychological language would be described as perceiving a black object. After all then the plain man and the experts take opposite sides; and though the presumption would still be in favour of the latter, if there were no dissentients among them, the position of the minority, backed up as it is by common sense, may fairly claim consideration—the position, namely, that we do not see or sense 'black' as something positive in itself, but only perceive black things or forms. The position itself can be stated in a few sentences, but the arguments for the counter position require discussion in detail.

The primitive sensation of sight, it is assumed, consists only of the single quality we call 'light,' a quality which ranges in intensity from the zero of complete darkness—as an ideal limit—up to a dazzling brightness that becomes painful and blinding. The first responses to light stimulation seem to be very much on a par with our own to diffused heat or cold: some creatures seek the light and others avoid it; the worm, for example, on a sudden flash of light withdrawing into its hole, and the *Pholas* into its shell. As little as our temperature sense yields us a perception of form does the light sense, at this level, yield any. Only when the stage of visual spatial perception is reached, and some discrimination of form is possible, do black and white attain the meaning they bear for us. In ordinary language—primarily at any rate—we apply these terms only to shapes or 'things': to use Helmholtz's terminology, they are 'body-colours[1].' A coloured object can only be perceived when its colour differs from that of the surrounding visual field: so far black as a 'secondary quality' is on a par with other colours and for *practical* purposes would be equally entitled to the name, even if there were black objects devoid of all lustre and absolutely absorbent of light. But there is still an important difference: in a light-field many colours may be distinguished, but in a dark-field none. Though it is correct to speak of *perceiving* a black object, must we not then maintain that the object, so far as it is really black, yields us directly no *sensation*? Its so-called 'black' colour answers only to a portion of the dark field, and with this *causa deficiens* on the sensation level—to adopt an apt comparison of Meinong's—there corresponds a positive percept only because some form or other is demarcated by the rest of the visual field which does yield positive sensations. Similarly the piper is said to 'feel' the holes in his whistle when actually he only touches the solid metal in which they are pierced; or the soldier is said to hear the tattoo though he has no auditory sensation of the silence intervening between successive taps on the drum. An obvious means of differentiating between 'positive' and 'negative' sensations here suggests itself:—The order in which the first occur is immaterial, but the second, that is the absence of certain sensation, can only be experienced when preceded by their presence. We can begin either with, say, rough or smooth, bass or alto, red or blue. We cannot begin by experiencing the impalpable, the inaudible, or the invisible.

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[1] *Physiologische Optik.* 2te Ausg. S. 822.

Is there then any justification for speaking of visual sensations without luminosity: must we not rather maintain that in darkness, subjectively and objectively absolute, we should not *see* black, since we should not see at all? No doubt we are prone to identify the two concepts, darkness and blackness, for what we may call their sensory content is the same—viz. the absence of visual sensation.

Even Helmholtz seems to do this, regardless of his own distinction between variations of light-intensity and body-colours. "Black is an actual sensation," he says, "*i.e.* perception of a definite state of our organ, even though it is brought about by total absence of light[1]." A black state may be brought about in a photographic film by the presence of light; would Helmholtz have said that it is to this state that our organ is brought by the absence of light and that the perception of both is alike? Not only is it obviously inaccurate to speak of blackness as a state of the eye, but even the propriety of calling darkness such a state is very questionable. What it naturally occurs to everyone to ask, however, is why the resting state of the eye is said to be accompanied by a positive sensation when nothing of the kind is asserted of the like state of other sensory organs. By parity of reasoning it would seem we ought to maintain that silence is a positive sensation of sound. But *à priori* considerations, it must be allowed, are not decisive here: it is a question of fact. True, it is replied, and as a matter of fact darkness is not the concomitant of a resting state of the visual apparatus, and thus there is no parallel between darkness and stillness. Leaving physiological questions aside for the present, let us see what are the psychological grounds for denying this parallelism.

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[1] *Physiologische Optik.* 2te Ausg. S. 324.

"It seems to overlook a very essential point," says Professor Stumpf. "There remains the great difference, that the ear in the absence of external stimulation at all events approximates to the limit of not hearing, while in the analogous case of the eye, the like by no means happens.... We see *nothing*, *i.e.* no external objects—that is clear, but we are far from admitting that we do *not* see in the sense in which we admit this as regards our back or our feet. We are just as distinctly conscious of a sight sensation as we are when the eyes are open, but of sensations of hearing, when it is still, we are in the first instance not conscious at all. The retinal black cannot be described as an extremely feeble sensation; and there is no need, as in the case of the faintest sounds, for highly strung attention in order to perceive it at all. The deepening of black makes the sensation rather stronger than weaker, whereas in the transition to deeper silence, the intensity of the sensation decreases[1]." It is certain that in the dark we should not admit that we cannot see in the sense in which we admit this as regards our hands and feet. The fact that we are directly aware whether our eyes are in the dark or not establishes this difference, but is no proof that darkness is as distinctly as light a positive sensation. *Mutatis mutandis*, the same may be said of hearing. When there is nothing to hear we do not say that we do *not* hear in the sense in which we say this of hands and feet. The difference between privative terms and negatives is surely obvious. Certainly 'retinal black,' *if it is a sensation at all*, cannot be described as a feeble one. The interest and the constancy of visual sensations throughout waking life naturally make their cessation impressive and it must be admitted that the cessation of sounds is not often equally striking[2]. But whether it is true that it is harder to convince oneself that a particular darkness is absolute than that a particular silence is so, is surely an open question, and perhaps not an important one. At any rate when we make the attempt what we are intent upon in the one case is light and in the other sound; and if these are not completely absent both alike become fainter as we approach the limit, while the deepening, whether of darkness or silence, *if these are sensations at all,* yields a stronger rather than a weaker sensation. We cannot fairly parallel the diminishing sounds in the one case and the increasing darkness in the other. But it is doubtless true—and this perhaps is the point underlying Professor Stumpf's reasoning—that black has an objective significance to which silence furnishes no analogue: hence we have no special term for objects that emit no sound and indeed no experience of such. It is only too easy to confound this positive significance for perception with positive reality for sensation, as ordinary thought and language invariably do; and it is difficult to avoid the suspicion that even psychologists of Professor Stumpf's eminence[1] have been misled by this confusion into begging the question they mean to prove.

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[1] *Tonpsychologie*, I. S. 381.

[2] Yet there are some cases in which it is, and then everybody speaks of the cessation as audible. It is this perception of silence that wakes the miller when his wheel stops and the clerk when the sermon is ended. Cf. McDougall, "Young's Theory of Light- and Colour-Vision," *Mind*, N.S. X. p. 96.

We seem to have only an instance of this confusion in the following further statement of Helmholtz:—"We are very well aware when the eyes are closed that the black field of sight has a boundary; we by no means allow that it extends away behind our back[2]." But when, as here, we are dealing exclusively with sensations, we have no right to talk either of a 'field' or a boundary. Such terms imply spatial perception. A sensation may have more or less extensity but this finite quantity is not the same as bounded space. The extensity of a sensation, like its intensity, is intrinsically limited, not extrinsically, that is to say it is not bounded: a mere sensation of a certain extensity does not 'march' with another of like or different modality. What we are immediately aware of, when we close the eyes on a pitch-dark night is not absolute black, since the internal retinal light is still present; and so long as we attend to this and its continual changes so long we are aware of positive sensations and a certain extensity. But to disregard this, as we commonly do in speaking of such a state as 'total darkness,' and yet to ask where this darkness is localised and what it includes or excludes, seems unmeaning. Still at least it must have some extensity, it may be said. Certainly, provided it have some quality and intensity, but this is the first question. But in truth extension and not mere extensity was what Helmholtz meant; the word he uses is 'sensation *of* black,' the fact he describes is the perception of a black 'field.' And everyone must allow that when we have advanced to this perceptual level we can discriminate the form of a black object on a white ground as well as that of a white object on a black one; and if, when the white object fills the whole field, this is still regarded as extended, why, it may be urged, should not the same hold of the dark field? But even so, if no positive sensation pertains to a black object, why should any be assigned to the black-field? What more have we than a percept of visually empty space? It seems useless then to attempt to find an answer to our question by appealing to perception[1]. A reference to the definition of field of sight which Helmholtz gives in the section devoted to perception (pp. 673 ff.) is conclusive evidence of its perceptual character. In this section too there is a later passage, which, if it stood alone, would be entirely satisfactory:—"The sensation of darkness is the sensation of the resting state,—or if it be preferred—is the absence of sensation in parts of our nervous apparatus for vision, which would be excited if a stimulus were applied to them. To it there corresponds in perception the idea (*Vorstellung*) of parts of space in front of us, that send out no light to our eye; which therefore implies a definite, though also negative, predication concerning the objective state of this portion of space," p. 717. Equally explicit is the following passage—*à propos* of Hering—added to the second edition:—"This perception that a sensation [of light] that might be present, at the moment is not, involves after all a predication concerning the existing state of the organ...and *in this sense* we designate that state too a sensation, viz. one of darkness," p. 379. On the whole Helmholtz may fairly be counted on the side of the minority.

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[1] An equally eminent physiologist—Professor v. Kries, *so far as he shares the common opinion*—seems to lay himself open to a like suspicion. "Unbiassed introspection will never allow black to count as a lower degree of intensity of the sensation of white," he says in one place; but in another, "The unbiassed will always be inclined here also [when the eyes are closed] to speak of a cessation of seeing, as absence of sensation (*Nichtempfinden*)." Both statements are true: to the unbiassed mind, black is positive, *i.e.* for perception, and darkness negative, *i.e.* for sensation; but the unbiassed mind would prefer to say not that black is a lower degree of the sensation of white, but that dark is a lower degree of the sensation of light. Cf. Nagel's *Handbuch der Physiologie*, III. pp. 136, 273.

[2] *Op. cit.* S. 324.

Professor Hering is the chief champion of black as a positive sensation, and he at least is clear of the confusions just referred to between sensation and perception. For him the body-colour black is a positive sensation because in his opinion the facts of light-induction and contrast compel us to regard darkness as one. The so-called sensation of the deepest black he has shown to be possible only under the influence of light. White and black are then, he argues, on a par: in so far as the retina must be partially illuminated if the deepest black is to be seen (*empfunden*). The only difference is that in the case of black the influence of the light is indirect—a most important difference surely. In the dark, that is to say in the absence of all external stimulation, there is always present more or less internal stimulation—the so-called intrinsic light of the retina (*Eigenlicht*). Let us suppose we succeed in finding a dark grey to match this: a strip of such grey on a white ground yields a sensation that is darker still. In place of the psychological effect of contrast to which Helmholtz alone appealed in explanation of this fact, Hering lays stress on the 'negative induction' of the white ground as a purely physiological effect: the strip does not merely seem darker by contrast but the sensation actually is 'darker' in itself. But Hering does not expressly deny the presence of psychological contrast as one factor in the result: at any rate this is unquestionable. The simplest statement we can make of the psychological effect of the 'negative' and seemingly inhibitory neural process, and the statement most in accord with the facts of sensation generally, is to say that this effect too is negative: the sensation is reduced still further towards the zero of absolute darkness and the contrast so much the more enhanced. The fact that part of this total effect is due to the indirect physiological influence of the light stimulus simultaneously present is, so far as psychological description is concerned, wholly irrelevant. Leaving physiological differences aside, we can at least clearly imagine a parallel in the case of sound, which would hold good psychologically at all events. Suppose we hear a loud sound of constant intensity and quality and simultaneously a much fainter one of different quality. Let the intensity of the latter steadily diminish and the contrast in intensity between the two sounds will steadily increase; and there is *à priori* no reason why we should not attribute this growing contrast to the increasing stillness of the waning sound. That there are empirical reasons why we do not is undeniable; but these reasons are more physical than psychological. Muteness or 'insonance'—to coin a word—is not a secondary quality of objects as blackness is, nor do auditory sensations develop spatial perception as visual sensations do. But it is not difficult to imagine modifications in the two senses—and perhaps in the environment—which would bring about an exchange of *rôle*, and then it would be natural to regard blackness as negative and muteness as positive. Whereas seeing is preeminently the space sense, so hearing is above all the time sense, and here accordingly silence does assume a *rôle* analogous to that of darkness in the case of space. So we find Preyer saying: "Just as black is a sensation indispensable to painting so are pauses indispensable sensations for music[1]"; and Mach, followed by v. Ehrenfels, paralleling time-shapes with space-shapes[2].

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[1] Moreover, the parallel supposed is by no means exact. The dark-field is presumably coextensive physiologically with the retina—is bounded, that is to say, by the *ora serrata*—whereas the light-field falls within this. To this physiological difference there is an answering psychological difference. What we may call the spatially developed portion of the light-field, to which alone the term 'field' is appropriate—more particularly that portion the projection of which is ascertained by means of the perimeter—has a positive, though not sharply defined, boundary in the sensational fringe into which it rapidly merges. This boundary is lacking to the dark-field as such.

But Hering, who starts by considering not the simple fact of darkness itself but the more complex fact of its contrast with white, is led to regard it not as a decrease in, or the zero of, luminosity, but as a positive intensity increasing towards the maximum which he calls pure black, and as thus completely on a par in respect of reality with its contrary, pure white. Even if this were true, it is very questionable whether the fact of psychological contrast would be any proof of it—and of contrasts other than psychological we have no right to speak. In physiology and physics the appropriate term, and also the usual one, is not contrast, but opposition or antagonism: both of which, of course, imply two real processes. But contrast is possible when only one member of the relation is positive and answers to an existing stimulus, the other being its zero and answering to none. The change in consciousness is equally impressive and positive—as a change—whichever way it occurs. A blind man is unaffected by the sudden alternation of light and darkness: to experience darkness we must experience light too. But how does this prove that darkness is a sensation of equal intensity but opposite quality to light[3]? Yet this is Hering's view. The difficulties it entails are many and great.

In the first place, as Fechner and Fick have urged, if black and white, darkness and light, are alike positively intense, why are they not equally exciting and exhausting? A patient with sensitive eyes, to whom a faint light is painful, might only make bad worse—momentarily at any rate—by shutting out the light altogether. Light admits of indefinite increase, but who will maintain the like of darkness: who ever heard of a blinding black[4]?

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[1] *Ueber die Grenzen der Tonwahrnehmung*, 1876, S. 67.

[2] Mach, *Die Analyse der Empfindungen*, 1te Aus. 1886, S. 104; v. Ehrenfels, "Ueber Gestaltqualitäten," *Vierteljahrschr. f. wissenschaft. Phil.* 1890, S. 268.

[3] It does not *follow*, as Professor G. E. Müller seems to argue (*Zeitschr. f. Psych. u. s. w.* XIV. S. 25 f.), that because in the case of colour-contrast diverse qualities are involved, therefore they must enter also into brightness-contrasts, and mere difference will not suffice.

[4] It is true that Hering substitutes *Deutlichkeit* for *Intensität*, but this—as Fechner

Secondly, if black and white are in all respects antagonistic, then they ought entirely to neutralise each other when combined in a certain proportion—as is the case with complementary, or rather, antagonistic colours, as Hering prefers to call them. In place of this, however—though there are no red-greens or yellow-blues—there is a continuous scale of black-whites, and therefore a neutral or medium black-white (or grey), in which the two colours concur to an equal extent. This difference Hering attempts to explain by the hypothesis that red and yellow light act anabolically, green and blue katabolically, in producing the corresponding sensations; whereas all light is katabolic as regards white. If this were not the case, if mixed sunlight acted anabolically—producing black, as green light produces green—and if this anabolic action were as intense as the katabolic which yields white, "then," says Hering, "we should not see such sunlight at all[1]." But at all events, however produced, this assimilative process yielding black, according to his theory, actually exists, and in the medium grey it *is* present to the same extent as the dissimilative process which yields white. Why, then, is there any sensation in that case, when in the parallel case of two antagonistic colours there is none at all[2]? This serious inconsistency, which every critic of Hering's theory has emphasized, would be wholly removed by the recognition of the merely negative character of black. But there is another difference between the supposed antagonism of black and white on the one hand, and that of antagonistic colours on the other. Blue and yellow, though antagonistic, are otherwise so far independent that the intensity or the saturation of the one may diminish without that of the other increasing, or *vice versâ*. Whereas more white involves *ipso facto* less black, and *vice versâ*; and of saturation in connexion with them we have no occasion to speak at all. This seems to show not only that both belong to one and the same process, but to suggest the purely negative character of one of the two; if so, there can hardly be much question that black is the one.

Thirdly, since there are not two intensities of the same grey, Hering

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notes—is only an abuse of terms. If anyone chooses to say that the twilight glimmer differs from the noonday glare merely in being less *distinctly* light, we can only protest. The graphical and numerical exposition of his position which Hering gives (*Zur Lehre vom Lichtsinne*, § 22) shows clearly that he is dealing with what everybody else calls intensity.

[1] *Zur Lehre vom Lichtsinne*, S. 122.

[2] The subsidiary hypothesis of 'specific luminosities' which Hering afterwards advanced, even if verified, would not obviate the difficulty in question.

assumes that what we call its intensity is determined entirely by the relative, not by the absolute, amounts of its black and white constituents[1]. Such thoroughgoing relativity, without a parallel elsewhere, leads to extraordinary consequences when we come to consider the facts of visual adaptation, as Professor G. E. Müller has already pointed out[2]. The pitch-blackness we experience on first entering a cave disappears at length as the eyes accommodate themselves; we then perceive 'the intrinsic light of the retina,' which is only a deep grey; on returning to the daylight in like manner the dazzling glare is after a time exchanged for distinct vision as the eyes re-adapt themselves to the increased stimulation. In both cases, then, a stationary state is eventually reached, in which, to use Hering's language, "a complete balance between assimilation and dissimilation is attained"; in both cases, that is to say, the ratio of black and white is the same; and therefore the sensations ought to be identical. The wide difference that in fact obtains between them, though a refutation of Hering's 'psychophysical principle,' shows indeed that there are not two intensities of a given grey, regarded as a combination of black and white, as there may be two intensities of a given clang. But it also shows that the assimilative process, which has the same ratio to the dissimilative in both cases, cannot be the 'cause' of a positive sensation of black; for, if it were, there would be *two* intensities of the same grey.

Fourthly, the meaning which Hering assigns to 'medium grey' is very different from the ordinary one. The plain man would tell us that he knows nothing of a fixed medium grey at all, since he knows nothing of a fixed, absolute black or white. But, given a certain illumination, he would probably understand 'medium grey' to mean the body-colour that appeared equidistant between the two taken as the respective standards of black and white. For Hering, on the contrary, medium grey is not a body-colour at all, but what we see "on awaking during the night in a pitch-dark room." The 'intrinsic light of the retina' that then obtains "must, according to my theory," he tells us, "be the medium grey, for the assimilative and dissimilative processes being then approximately equal, the sensation must be about equally distant from those of absolute black and white[3]." That facts *seem* to contradict his theory he candidly allows. But why is the contradiction only apparent? Not because it is not really the fact that the darkness

[1] *Op. cit.* S. 55 f.

[2] *Zeitschr. f. Psych. u. s. w.* XIV. S. 65.

[3] *Op. cit.* S. 88 f.

of the adapted eye is nearer to the intensest black we can experience than it is to sunlight, but solely because *if* there were actually light rays working assimilatively on the retina with the same intensity as that of the rays which work dissimilatively "we should necessarily have a sensation of black, which in depth or intensity would exceed quite extraordinarily the deepest velvet black actually experienced and be altogether blinding." Yes, *if*; but, as Hering naïvely adds, "there are no such rays...and according to my theory there cannot be[1]." The 'intrinsic light of the retina' is then a medium grey only in the sense of lying midway between absolute white and a "merely conceivable" absolute black, of which we neither have, nor can have any experience.

Finally, it may be doubted whether it is even physiologically conceivable that an anabolic or constructive process should ever attain the intensity possible to a katabolic or destructive one. Moreover, not only is it *à priori* very questionable on biological grounds[2] whether anabolical processes ever entail *specific* sensations at all; but, further, there is no clear evidence that in fact they do[3]. *If* black or darkness is a positive sensation there must be a corresponding neural process, that is certain; but Hering's assumption that this is to be found in the assimilative process that makes the sensation of light possible seems to be an assumption merely, and an assumption too which the negative character of darkness would render altogether needless. It is also certain that the dissimilative process must be accompanied or followed by an assimilative one; but again it is an assumption pure and simple that there must be an answering sensation, viz. black.

Since the appearance of Hering's classical contributions Wundt too has been a prominent champion of black as a positive sensation. But if anything were calculated to make an unbiassed reader sceptical, it would be the examination of Wundt's various expositions. In the earlier editions of his *Physiological Psychology* he represented the whole series—white, grey, black—as simply an intensive series, in which white passes through grey into black, "its lowest degree." In the later editions, after proceeding as before, he suddenly turns round, saying: "Nevertheless, it is unquestionable that at the same time we subjectively experience (*empfinden*) black and white as qualitative opposites[1]...." Their relation to bright and dark *objects* may have helped to fix the distinction, but is insufficient to account for its origin, since we also ascribe black to the dark field of sight, from which all objects are excluded[2]. It is true that Fechner has used the term *Augenschwarz* for what, since Helmholtz, is generally called *Eigenlicht*. But the term in this sense Wundt stigmatizes as "wholly unwarranted": the black that he intends is neither "white nor coloured[3]," in short, it is the absolute black that, according to Hering, we never actually experience at all. But now it is surely notoriously not true that we ascribe such a black to the 'dark field,' and still less true that we do this before we come to distinguish objects as black or white. Wundt wholly ignores the important difference on which Helmholtz, and even Hering have insisted, between mere light or shade as grades of 'luminosity' and black or white as body-colours or secondary qualities of objects. It is just because of this difference that we can ascribe black to a 'bright object' and white to a 'dark' one, when—recognizing the objects—we take account of their different illumination. That Wundt is unwittingly talking the language not of sensation, but of perception, is manifest from his phrase, "*ascribe* black to the dark-*field*." Turning, however, to his description of this sensation in detail, we come upon fresh difficulties.

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[1] Cf. above the passage quoted on p. 415.

[2] Cf. Fick, *Sitz. Ber. d. Physik. Med. Ges. zu Würzburg*, 1900, S. 9–15; G. E. Müller, *Zeitschr. f. Psych. u. s. w.* XIV. S. 74.

[3] In his theory of the temperature-sense Hering in like manner assumed two antagonistic processes: had this assumption been confirmed by recent experiments, it would have afforded some support to the corresponding theory of the light-sense, for cold is unquestionably a positive sensation.

First, we have an intensive series, ranging between 'absolute black' and 'absolute white,' though, since black is always present, there is really no absolute white. The intensity here diminishes with the intensity of the light-stimulus till we reach a threshold which is called "the darkest white" (*das dunkelste Weiss*)[4], while the final limit of the series is called "the darkest black" (*das dunkelste Schwarz*)[5]. What we wonder is the relation between these two, and which of them corresponds to "the lowest intensity of white"? If there are *sensible* degrees of black, wherein can their difference lie? If it be in the amount of white present, and this on the whole we must suppose so long as the light-stimulus can be further diminished, then the threshold reached was not the darkest—why not say the blackest?—white. In that case the blackest white must itself be black, the zero limit of the series, as Wundt himself has said; and then it only escapes becoming a contradiction by being the nonentity which zero implies. But the series is also a qualitative series, Wundt now maintains, and so regarded, black—in spite of its being the lowest (or zero) degree of white—is a positive sensation, in quality the opposite of white. To say nothing further of the anomaly of a sensation that has a positive quality, but no intensity, it is enough to observe that from the mere fact, if fact it be, that the series is a qualitative one it does not follow that both extremes are positive. We can imagine a series of tones, for example, rising (or falling) continuously in pitch and diminishing continuously in intensity; the limit of such a series would be silence, and this, it is allowed, is not a positive sensation. But black is equally the limit of innumerable qualitative series, beginning with red, yellow, green, etc. It can hardly be at once a positive quality and the precise opposite of them all; and if it is really the precise opposite of white, this difference would only tend to show that the white-black series *is* a purely intensive one, and black its limit or 'lowest grade.'

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[1] Yet he had himself seriously questioned, and even rejected this view in an earlier work. Cf. "Die Empfindung des Lichts und der Farben," *Phil. Stud.* IV. S. 360 f.

[2] *Grundzüge der physiologischen Psychologie*, 4^{te} Ausg. I. S. 504; 5^{te} Ausg. II. S. 162, slightly altered.

[3] And yet it is always present when a white or coloured impression is sufficiently *diminished*, is, in fact, the "lowest intensity-grade of white," *loc. cit.*

[4] *Op. cit.* 4^{te} Ausg. I. S. 508. This term is omitted in the last edition, though the thing is still implied.

[5] *Op. cit.* 4^{te} Ausg. I. S. 558; 5^{te} Ausg. II. S. 245.

Nevertheless, it is, Wundt maintains, an "indubitable fact" that the series is also one of qualitative differences. Let us turn, then, to his account of this second aspect of the series and see how it squares with the first. In place of beginning with white as a positive quality, diminishing in intensity as the light diminishes till black is reached when all light is gone, he now begins with black as a positive quality, with which, as such, the presence or absence of light has nothing at all to do. Its physiological counterpart is taken to be "a permanent internal excitation (*innere Dauererregung*) of the retina, which accompanies and outlasts all other excitations": hence all our sensations of light and colour are mixed with absolute black. At first Wundt regarded the intensity of this black as constant, attributing all the variations of intensity in the black-white "mixture-system," (*Mischsystem*) as he calls it, to varying external light-stimulations: there were "no independent degrees of intensity in black." On this hypothesis the intensive and the qualitative aspects would become identical—in other words, the qualitative aspect would become an intensive aspect simply—*provided the constancy of absolute black were the constancy of zero.* And a very little reflexion will lead most people to suspect that an excitation of the retina that is always present, always constant, and "involves no exhaustion" is an *Unding* that can only be equated to zero[1]. At any rate, Wundt himself seems to have come at length to this conclusion, for he now—*more suo*—silently drops his first hypothesis, without considering it necessary to refute the arguments he had previously advanced in its favour[2]. But according to his latest view the intensity of the black complement in his *Mischsystem* varies as well as that of the white[3]. But now he ought no longer to refer the gradations of the system exclusively to variations in the intensity of the external excitations caused by light, as he none the less continues to do. In fact, there is no longer any warrant for speaking of the series as a continuously graduated intensive series at all. For aught that appears to the contrary, the intensity of the 'system' may now be uniform, the intensity of one component increasing as that of the other diminishes, as Hering indeed assumes. One cannot run with the hare and keep with the hounds: Wundt, having at length parted from Helmholtz and sided with Hering, must perforce accept the consequences. For example, there cannot be two intensities of the same grey, Wundt says—as we all say—and this is obvious so long as the intensity of a given grey is regarded as determined exclusively by the intensity of the external excitation. But if we elect to regard the black due to internal excitation as also a factor and an independent one, then we can only maintain that there cannot be two intensities of the same grey, if, as Hering does, we are consistent enough to maintain also that the character of the grey depends only on the ratio, not on the absolute amount, of the two factors. Is Wundt prepared for this?

Matters are not mended when we find that Wundt, like Hering, attributes the internal excitation on which black depends to an anabolic process, and regards this as inhibitory in its action[4]. But, so far as it is inhibitory, the process cannot be a permanent and independent one, as Wundt assumes: it must presuppose the external excitation, on which the sensation of white depends. And it must neutralise this more or less completely[5]; in a word, its action must be negative, and equally so the sensation it is supposed to induce. But why a fundamental, vital process should yield specific sensations, to say nothing of doing this only in the case of a single sense, Wundt, as little as Hering, attempts to explain.

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[1] The sensation corresponding to absolute black will then be "the null-point" of the whole series, and as such Wundt in the very same context actually describes it.

[2] Cf. *Phil. Studien*, IV. S. 365–367.

[3] *Psychologie*, 5te Ausg. II. S. 168.

[4] *Op. cit.* 5te Ausg. II. S. 242, 248.

[5] As already said, Wundt in his article (*Phil. Studien*, IV. S. 360 ff.) controverts Hering's view concerning the primitive antagonism of black and white as sensations;

In his remarkable articles *Zur Psychophysik der Gesichtsempfindungen*[1] Professor G. E. Müller exposes and endeavours to remedy the defects of the Hering theory. But he is so completely in accord with Hering on the question now before us that he never for a moment pauses to substantiate the claim of black to the character of a positive sensation. The "altogether peculiar phenomenon," as Wundt allows it to be, that black and white form not merely an intensive but also a qualitative series is for him too 'an indubitable fact'; so much so that in spite of its peculiarity and its difficulties, it is one he finds it needless scrupulously to verify. And yet he escapes these difficulties as little as his predecessors, although his main concern is "to modify the theory of antagonistic colours so as to explain the quantitative singularity of the black-white sensations without the help of Hering's unwarrantable [psychophysical] principle[2]"—to explain that is to say the existence of a graduated series of black-whites (greys), though there is no corresponding series of red-greens or yellow-blues, without the assumption that one and the same grey may result from different intensities of black and white, provided their ratio is the same.

For Hering's antagonism of anabolic and katabolic processes Professor Müller substitutes the more rigorous antagonism of reversible chemical or molecular actions; and whereas Hering leaves it doubtful how far his antagonistic processes are peripheral or central, Müller distinctly assumes that his own reversible actions take place both in the retina and in the central 'visual substance.' But he distinguishes carefully between the processes occurring in the retina proper and in his subcortical or cortical retina—as we may fairly call it—on the one hand and the nervous processes by which they are connected on the other. Further he supposes that during the resting or neutral state all three antagonistic processes (white-black, red-green, yellow-blue) presumably occur, alike in the retina and in the central visual substance, as a consequence of the molecular 'collisions' due to incessant thermal disturbances; the several antagonistic pairs being very approximately

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and to the last charges him with inconsistency in maintaining this antagonism of black and white, while yet regarding grey as a compound of the two. But now we have Wundt himself asserting both!

[1] *Zeitschr. f. Psych. u. s. w.* 1896–7, Bde. x. and xiv.

[2] *Op. cit.* xiv. S. 62.

in equilibrium and of constant intensity, since the temperature is practically uniform. When the balance between two reversible or antagonistic reactions in the retina is disturbed, only the intenser of the two produces a nervous excitation; when both are equal therefore the retinal process does not affect the corresponding central process at all. In the case of black and white then, when a nervous excitation occurs it is one which would of itself give rise either to a sensation of white or to a sensation of black, but not to a mixed sensation of grey. Thus, if these centripetal excitations were all, we should experience pure black and pure white varying in intensity, as hot and cold, for example, do; but we should know nothing of a graduated series of greys. But on Müller's theory, as we have just seen, they are not all; in the central visual substance during its resting phase the black and white reactions still occur, though they balance each other. Here, however, these reactions are not merely physical processes as in the case of the retina, but psychophysical; in other words, the reactions are *ipso facto* excitations, 'endogenous excitations,' as Müller calls them. Hence the so-called, and in fact if the theory be sound the mis-called, 'self-light' of the retina or *Augengrau*. So Müller works his way to what, like Hering, he regards as the medium-grey; but it is a centrally, not a peripherally, excited sensation. And now, when say the retinal W (white)-process predominates the excitation consequent upon it, on reaching the central visual substance, increases the W-process and excitation and diminishes the B (black)-process and excitation that are there taking place: so, *mutatis mutandis*, when the retinal B-process predominates. Thus the existence of a graduated series of black-whites (or greys) is supposed to be explained.

But not only are the assumptions underlying this explanation questionable; there is also at any rate one difficulty inherent in the explanation itself. The series of greys obtained we may represent by means of the following figure, in which perpendiculars above XX' stand for white intensities and perpendiculars below for black, MG being the medium grey of the visual centre when free from peripheral excitations. So long as the components of this grey last, a series of continuously increasing white excitations will yield a corresponding series of light greys and a similar series of black excitations a series of dark greys, the limit of the one series being pure white, that of the other pure black. But it is reasonable to assume that the intensities of both

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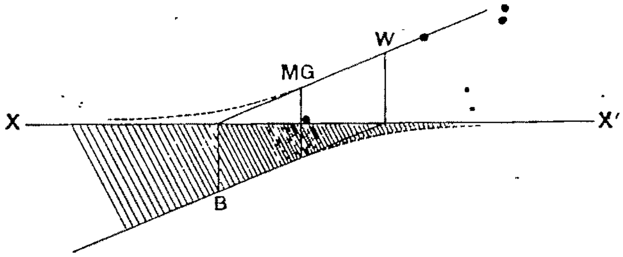
[1] *Op. cit.* XIV. S. 21, 25.

components of *MG* are small—and Müller himself speaks of them as "possessing a minimal value ($W_0 + B_0$)[1]," and so it is highly probable that excitations from the retina may arrive which will practically more than exhaust one or other of them—in fact if photometric estimates of the intensity of the 'subjective grey' are any evidence, this would certainly happen. In that case we should have beyond the series of greys an indefinite series of pure whites of varying intensity on the one side and a corresponding series of pure blacks on the other, as indicated in the figure. But this would conflict with the other position, viz. that the black-white series is wholly a qualitative series or '*Mischsystem*,' which Müller in common with Hering and Wundt strenuously maintains. Indeed he calls the series in distinction from the series of tones "essentially limited (*prinzipiell begrenzt*), because we cannot conceive the change consisting in the sensation becoming blacker or whiter prolonged beyond pure black and pure white[2]," *i.e.* beyond the lines *B* and *W*, where the white and black components respectively vanish. Not only are these limits for Müller essentially impassable, but like the rest of us, he seems inclined to question whether they are ever even attained, whether, that is, we ever experience pure white or black at all[3]. For those who regard the white series as purely intensive there is no difficulty in this: it simply means that we have no experience of either the zero or the maximum of the series. But for Müller the difficulty is serious, assuming, as he does, that excitations having a considerable range of intensity but answering only to pure white reactions or pure black reactions are sent up from the retina and, on arriving at the visual centre, are only prevented from becoming sensations of pure white or black by admixture with the endogenous black or white—described as 'minimal' to start with—which diminishes steadily in intensity as their intensity increases. To represent this



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[1] *Op. cit.* xiv. S. 85.

[2] *Op. cit.* x. S. 84.

[3] *Op. cit.* x. S. 34, xiv. S. 85 n.

diminution as asymptotic (shown by dotted lines in the figure) will not practically mend matters; for when in the fraction $\frac{w}{w+s}$ s is very small compared with w, the value of the fraction is indistinguishable from 1. In short it would seem that Müller's theory only avoids the difficulties familiar in Hering's by failing to account for the series of greys as we know them.

But there is another difficulty, and perhaps a more fundamental one. Granting that 'endogenous' W- and B-reactions and -excitations take place, why should they—nay, how can they, blend to form grey? A 'mixed sensation' is either psychologically meaningless or it answers to what is really a sensation-complex, such as a clang for example; and the sole evidence for its complexity is the possibility of 'psychical analysis.' But grey is not in this sense a mixed sensation, it is not psychically analysable into black and white. All this Professor Müller admits[1]. All the mixture there is then is a mixture of excitations and it is solely in this psychophysical sense that Müller uses the term 'mixed sensation.' But how can equal antagonistic excitations combine to produce a positive result instead of neutralising each other? If they cannot coexist in the same nerve fibre, why should they in the same neuron? Müller himself—*à propos* of binocular colour-mixture—seems to admit this difficulty. "The occurrence of a yellow-blue sensation," he says, "is entirely excluded; for that would presuppose that excitations due to antagonistic forces... could be transmitted together into one and the same beat (*Bezirk*) of the nervous system[2]." But if they cannot be transmitted there how can they originate there? The reactions are reversible chemical or physical processes taking place to an equal extent in consequence of irregular heat-encounters throughout a substance free from external disturbance: that is to say we have a case of dynamic equilibrium, and no external work is done. If we imagine a Maxwellian demon ubiquitous throughout the substance, to him the reactions might still furnish excitations, but each excitation would be a distinct event: there would be no reason for two antagonistic ones to blend. And apparently it is something of this sort that Müller does imagine, for—after describing the neutral state of the visual substance as one in which there is no change of energy—he adds, "but in which from the molecular mechanical standpoint many things happen that for psychophysics are by no means without significance: on the contrary they form the basis of the sensation of subjective grey (*Augengrau*)[1]."

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[1] *Op. cit.* x. S. 14.

[2] *Op. cit.* xiv. S. 40.

But the qualitative 'singularity' of the black-white series is more fundamental than its quantitative and involves hardly less difficulty. Why are there not also central blends of blue and yellow, red and green? "Of course," Professor Müller remarks, "what has been said [of the grey series] is by no means meant to exclude the supposition that the endogenous excitations of the visual substance in principle [*im Grunde*] consist not only of black and white but also of four chromatic excitations as well[2]." But he insists that in point of fact there is, normally at all events, no evidence of the existence of these last. For the moment, however, we are not concerned with facts but with Müller's theory. The existence of the black and white endogenous excitations is hardly yet to be enrolled as a fact, and meanwhile the theory that suggests it must be consistent. Müller assumes that the 'strong preponderance' of the endogenous grey is such as to deprive the chromatic blends of any appreciable effect. Some preponderance there is good ground for assuming in view of the preponderance of white in our visual experience generally; but that experience hardly suggests a preponderance that amounts to total suppression. In fact we seem forced to choose one of two evils: either the endogenous grey holds the field by reason of its great absolute intensity or it is so near the threshold that the red-greens and the blue-yellows are below it. In the first case the saturation of our colour-sensations would be affected, and in the second, as already urged, a complete series of greys would hardly be possible. If endogenous excitations occur, as they very well may, there is no reason to suppose that they differ essentially from the internal excitations to which in their absence the retina would give rise. Nor is there at present any sure means of separating the two. After long sojourn in the dark what we are aware of is best described as a fine and faint galaxy twinkling in many colours. Müller seems to regard his endogenous excitation as a uniform background of medium grey for all this 'light-dust' or 'light-chaos,' as it is sometimes called. Wundt's contention that this background is not grey but black seems nearer the truth, for when, as occasionally happens, one discerns starless patches in this 'dark-field' they seem comparable to the blackness produced by 'contrast' in Hering's experiments. The simplest explanation of both, it may be urged, is the temporary cessation or inhibition of the intrinsic light[1]: all which makes against Müller's endogenous grey, and supports the more common-sense view of the purely negative character of black[2].

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[1] *Op. cit.* XIV. S. 64, *init.*

[2] *Op. cit.* X. S. 344, *fin.*

Surely the simplest way is to let the facts speak for themselves. Blue and yellow are complementary or antagonistic, because they do not yield a graduated series by mixture as blue and green do: black and white are not complementary or antagonistic because they do yield such a series. The series intermediate between blue and green is a qualitative series because it depends on variations in two distinct stimuli and cannot be produced by varying one alone: the series between black and white is not a qualitative series, because it can only be produced by variations of the one positive stimulus, internal or external, yielding white. Blue and green are both positive sensations, because both have assignable stimuli and can occur in any order and independently: black is not a positive sensation, because it has no assignable positive stimulus and depends solely on the cessation of light, internal and external. On the whole then we may conclude that the older and simpler view that "darkness or black is nothing but the result of the state of rest of the whole or part of the retina"—as Johannes Müller puts it[3]—is not yet exploded, and that, for the present at any rate, it is therefore premature to claim for the opposite view now in vogue the dignity of 'indubitable fact.' Hering's many admirable experiments show indeed the insufficiency of Helmholtz's explanations of so-called simultaneous and successive 'contrast'; but the physiological processes of induction which they reveal cannot be said to shut us up to the acceptance of the positive character of black till his theory is cleared of the grave difficulties that at present beset it. It by no means follows that because Hering has refuted the Young-Helmholtz theory he has, *ipso facto*, established his own. If black is verily a positive sensation nothing should be easier than to show this beyond cavil psychologically and physiologically. But this has never been done. To retort that "to define it as the absence of all light-sensation is as useless as to define green as the absence of all red or the sphere as the absence of every other figure[1]," is a palpable begging of the question, evidence not of a strong case but of a weak one. Nobody would object to defining silence as the absence of all sound till he had shown beyond question that it was something more. Nothing is commoner than to find that an intractable problem involves a false assumption—like the famous poser submitted by Charles II to the Royal Society, 'Why does a kettle of fish weigh more when the fish are dead?' disposed of by the discovery that they don't. Similarly the problem of black as a positive sensation may yet be as simply disposed of by the discovery that it isn't.

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[1] Cf. McDougall, "Young's Theory of Light and Colour-Vision," *Mind*, N. S. x. pp. 58, 96.

[2] *A propos* of an experiment which he has described (xiv. S. 85) Professor Müller maintains that it is "altogether inconceivable" (S. 86 n.) how this experiment is to be explained without assuming the positive character of black. Yet Professor McDougall (*op. cit.* p. 94) seems to have done this satisfactorily, and even to have turned the tables by experiments of his own which give results incompatible with theories of a special black-process.

[3] *Physiologie*, 1846, Bd. II. S. 296.

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[1] *Op. cit.* S. 68.

THE ILLUSION OF THE 'FLUTTERING HEART' AND THE VISUAL FUNCTIONS OF THE RODS OF THE RETINA.

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Two distinct illusory appearances commonly confused. The one is observed in bright light only, the other in dim light and with dark-adapted eye. Observations supporting the explanation of the latter appearance suggested by von Kries. The bearing on theories of the visual processes.

In this short paper I wish, first, to point out that two quite distinct illusory appearances have been hitherto confused and brought together under the designation 'the fluttering heart'; secondly, to show that a suggestion thrown out by Prof. J. von Kries affords the true explanation of one of these.

Helmholtz wrote[1], "A singular phenomenon, which perhaps belongs here, is the 'fluttering heart.' Figures of lively colours are fixed upon other-coloured sheets of stiff paper; red and blue seem to work best, the colours must be very bright and saturated. When one looks at the sheets and moves them to and fro with a certain rapidity, the figures seem to move relatively to the paper itself and to oscillate to and fro upon it. The cause of the appearance seems to be that the light sensation in the eye does not begin and cease with equal rapidity in the case of the different colours, and that on this account the blue seems to lag somewhat behind the red in the path described by the sheet."

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[1] *Physiologische Optik*, 2te Ausg. S. 588.

This is the appearance to which, I think, the term 'fluttering heart' should be confined. It has been described anew by Dr A. Schapringer[1], who offers an explanation based upon the difference of refrangibility of the rays from the red and blue surfaces, in virtue of which the retinal images of all the red and blue parts of the surface, save those at or close to the centre of the field of vision, suffer a relative displacement which must vary in degree and direction as the coloured surface looked at is moved to and fro while the eye remains at rest. For the detailed exposition of this explanation I must refer the reader to the original paper. I will only add that it seems to me entirely satisfactory, except that in my own case the directions of the relative movements of red and blue seem to be the reverse of those demanded by this theory, a fact for which I can offer no explanation.

Schapringer refers to the fact that many observers have stated that the phenomenon is more striking when the parti-coloured surface is looked at under low illumination and with the peripheral part of the retina, and he attempts to explain away these statements. Here he falls into the error of confounding two different phenomena which, though superficially similar, are due to very different causes. The one described by Helmholtz and Schapringer is, as they have noted, best seen with red and blue areas under strong illumination, and it seems certain that, even if Schapringer's explanation be not completely satisfactory, it is due to physical peculiarities of the refractive media and their spatial dispositions on the one hand, and the difference of refrangibility of the rays on the other hand, that in fact the explanation must be physical, and that it is not due, as Helmholtz suggested, to greater delay in the excitation of the sensation of blue as compared with the sensation of red. For in a paper in an earlier number of this *Journal* I have shown that there are no appreciable differences between the rates at which the sensations excited by rays of different refrangibilities follow the incidence of such rays on the retina, provided those rays are of such physical intensity as to excite sensations of approximately equal intensity[2]. The other phenomenon, clearly described by Dr A. Szili[3] and by Prof. Sanford[4], and named by them 'the fluttering heart,' is best seen when a surface made up of red and green areas is moved to and fro and the illumination used is low, namely, that of a single candle-flame at a distance of one to two metres. Sanford, writing of a diagram made up of blue rings on a red ground, says, "Use a small gas or candle flame for illumination, holding the diagram a yard or two from the flame.... Give the diagram a short side to side motion in its own plane, moving it three or four times a second, and notice an apparent slipping or springing of the rings from side to side. Close observation of the small circles of green or blue will show that it is not so much the coloured figures themselves that move, as it is a whitish shimmering image that seems to rest upon them, and to hang back when the diagram is moved. Notice that the apparent movement is most marked in indirect vision[1]." Szili describes the phenomenon in very similar terms; writing of a green disc on a red ground seen by the light of a single candle, he describes "einen hellen glanzähnlichen Schimmer, welcher mit den leisesten Schwankungen meiner Hand oder meines Blickes erzittert[2]." After describing a similar appearance with a number of different combinations of colours, Szili concludes that we have to seek the explanation of this appearance in colour contrast and in the projection of a negative after-image. Sanford concludes that "the apparent slipping is due to a lag in the retinal response—to a delayed after-image, as it were, but something different evidently from the ordinary after-image." Von Kries[3] on the other hand suggests that the appearance described by Szili is due to a difference in the time relations of the rod- and of the cone-excitations, *i.e.* that "the whitish shimmering image" of Sanford, or the "glanzähnlicher Schimmer" of Szili, is the image due to the excitation of the rod-apparatus, the organ for vision in a dim light, which yields only achromatic sensation, and that the lagging of this grey or whitish image relatively to the rest of the field is due to the fact that this rod-apparatus responds to stimulation less rapidly than the cone-apparatus, the organ for colour-vision and for achromatic vision in bright light. That this is the true explanation I have no doubt, and I wish only to supplement von Kries' brief remarks and Szili's descriptions with the following observations.

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[1] *Zeits. f. Psychol. u. Physiol. d. Sinnesorgan*, Bd. v. S. 385, "Zur Theorie der 'Flatternden Herzen.'"

[2] "The Sensations excited by a single momentary stimulation of the Eye," *Brit. Journ. of Psychology*, Vol. I. p. 105.

[3] "Flatternde Herzen," *Zeits. f. Psychol. u. Physiol. d. Sinnesorgan*, Bd. III. S. 359.

[4] *Experimental Psychology*, Boston, U.S.A. 1903, p. 318.

That the phenomenon described by Helmholtz and by Schapringer under the term 'fluttering heart' is quite distinct from that described by Szili and Sanford under the same name is clear from the fact that the former can only be observed when one uses saturated colours under strong illumination, whereas the latter cannot be observed under strong illumination and can be well seen only when the illumination is very feeble. Under these circumstances a patch of medium grey on a red surface exhibits the oscillatory movement almost or quite as vividly as a green patch, and rather better, I think, than a blue patch. That von Kries' explanation is the true one is further borne out by the following facts.

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[1] *Op. cit.* p. 319.

[2] *Op. cit.* S. 362.

[3] "Ueber die Wirkung kurzdauernder Lichtreize auf das Sehorgan," *Zeits. f. Psychol. u. Physiol. d. Sinnesorgan*, Bd. XII. S. 98.

(1) All the many combinations of coloured and achromatic surfaces, described by Szili as yielding the appearance, have this in common,—of each pair the one stimulates the cones, the other the rods more intensely. But a saturated red ground which, as has been shown abundantly, does not stimulate the rods appreciably, always gives the effect most clearly, and gives it best of all when it is combined with a patch or patches of green, which, as is also well established, stimulates the rods more intensely than light of other wave-lengths; *i.e.* the relative movement of the two surfaces appears most clearly when one of them sends to the eye light which stimulates the cones only, and the other reflects light which stimulates the rods most effectively.

(2) The amount of apparent slipping of the whitish film-covering the green or blue patch is proportional to the rate and extent of movement given to the surface. If the lateral excursion is made as rapidly as possible and of large extent (some three or four centimetres in extent) the whitish film appears for a moment separated entirely from the green or blue area by an interval filled with the red ground.

(3) The appearance of oscillation or shimmering or relative displacement is *not* produced when the green, grey, blue, or other rod-stimulating area is small and the eye is turned directly upon it, for then its retinal image being confined to the rodless foveal region of the retina, the rods are not stimulated by it. As regards this point my observation does not bear out the statements of Szili, who says that he cannot see the oscillation of the green patch when he looks directly at it on first going into a dimly lighted room, but does see it in direct vision after being ten minutes in the dim light. If the oscillation can be seen when the image of the green area is confined to the fovea von Kries' explanation is of course untenable. I have therefore directed special attention to this point. For this purpose a small patch of bright green paper, 5 mm. square, pasted on the centre of a sheet of stiff red paper of good saturation, is the most favourable object. When I hold

this steadily at arm's length in a dim light and fixate the green patch directly it appears as a very dark green on the dark red ground, and continues so to appear no matter how long I fixate it and how completely dark-adapted my eye becomes. And if I give the paper a slight side to side movement no oscillation of the green patch relatively to the rest of the surface appears. On the other hand, if I turn my gaze to a point on the red surface at a little distance (one, two, or more centimetres) from the green patch, it at once appears decidedly brighter and of much lower saturation, as though a bright grey translucent film had been put upon it, or better, perhaps, as if it had been rubbed with phosphorus. And the longer my eye has been shielded from bright light, *i.e.* the more completely it is dark-adapted, and the greater the accumulation of visual purple in the rods and the greater their excitability, the greater is the increase of brightness and loss of saturation of the green patch on turning my gaze from it to a point one or more centimetres distant from it. If while my gaze is thus directed to a point a little to one side of the green patch I give the surface a to-and-fro lateral movement, the grey film upon the green patch at once appears to oscillate, lagging behind the red surface and the dark green patch at each movement; and if then the eye is turned directly towards the green patch again, the grey film disappears at once and the oscillatory appearance is no longer seen. Several independent observers, who were ignorant of the theoretical questions involved, have confirmed this observation for me.

(4) The angular width of the central circular part of the visual field, in which the green patch exhibits no grey film and no oscillatory movement, corresponds as nearly as can be judged to the angular width of the rodless foveal region of the retina. I find that at a distance of half a metre it is 20 mm., or a little less, in diameter. That Szili failed to notice the absence of the oscillatory effect when the green patch is looked at directly seems to be sufficiently explained by the fact that he used patches at least 15 mm. in diameter, and that, as the object was held in the hand, it was presumably about half a metre from the eye. Under those conditions any but a very small lateral movement would cause the image of the green area to fall, partially at least, on the rod-containing circum-foveal region of the retina, and so give rise to the oscillatory appearance. Further, it is not easy to maintain the direct fixation of the centre of such an area in a dim light unless some practice be made with special direction of attention to this point.

(5) Schapringer has sought to explain away the fact that a dim light has been stated by several authors to be favourable to the observa-

tion of the 'fluttering heart' effect, by suggesting that the greater width of the pupil in dim light favours the relative displacement on the retina of images of areas of different colours. As I have already pointed out, Schapringer is attempting to assimilate to the true 'fluttering heart' phenomenon this very different appearance, due to the slower rate of response of the rods as compared with the cones; and this suggestion is easily shown to be pointless by putting a small artificial pupil before the eye, when the relative oscillation of the grey film appears as clearly as when no such artificial pupil is used.

(6) That the rate of response of the rod-apparatus is slower than that of the cone-apparatus, *i.e.* that the achromatic sensation excited by the stimulation of the rods comes to consciousness a moment later than the sensations excited by simultaneous stimulation of the cones, has been independently demonstrated. Von Kries has roughly estimated the delay of the rod-reaction at $\frac{1}{50}$ sec.[1]. By careful experiments directed to determine exactly the amount of this difference of rate I have shown it to be about $\frac{1}{18}$ sec.[2]. Such a difference seems to be ample for the production of the appearance of oscillation.

(7) If the eye is completely bright-adapted no increase of brightness and of saturation is perceptible when the gaze is turned from the small green area to a point one or more centimetres distant from it, and no relative oscillation of the area appears when the surface is moved to and fro. It is instructive to allow one eye (say the right eye) to become completely bright-adapted by a sojourn of some minutes out of doors under a sunny sky, while the other eye is kept covered and dark-adapted. On then returning to a dimly lighted room and observing the small patch of green on the red card with either eye alternately, the difference of brightness and saturation is very striking when it is seen in indirect vision, while there is no such difference when it is looked at directly. On doubling the green image by converging the optic axes or allowing them to become parallel, the images of the right and left eye respectively may be seen side by side and directly compared. When then the dark-adapted left eye is turned directly upon the green area (when of course the right eye receives the image of it on the peripheral retina) no difference of brightness or saturation is perceptible. When the right eye is turned directly upon the green area and the left eye receives its image on the peripheral retina, the difference of brightness and saturation is very striking. If both eyes

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[1] *Loc. cit.* S. 97.

[2] *Brit. Journal of Psychology*, Vol. I. p. 106.

are converged on a point on the red surface at a little distance from the green area, a rivalry of the two images may be observed; it appears alternately a dark saturated green and a bright greyish-green of very low saturation. After some minutes the right eye begins to see some increase of brightness and saturation of the green patch as the eye is turned from it, and after some ten or more minutes there remains no marked difference between the appearance of the green area to the peripheral retina of right and left eyes.

Although the observation of these very striking and pretty effects is an amusing parlour-game, it has considerable importance for the theory of vision. All the appearances described above find a complete and entirely satisfactory explanation in that theory of the function of the rods which regards them as the retinal organs of an apparatus specially adapted for vision in a dim light, and which we owe to Max Schultze, A. König, and von Kries. And that theory is borne out by a multitude of facts, facts of comparative anatomy, of natural history, histology, of physiology and of sensation, which all are harmoniously and satisfactorily explained by it. But the acceptance of this theory involves the giving up of much that has been held to be among the strongest parts of the ground on which rests Prof. Hering's theory of the visual processes; in fact this theory, which has become a dogma of the physiological schools, can hardly be reconciled with the new theory of the functions of the rods, now so well established; if we accept the one we must reject the other.

On the other hand, the acceptance of this theory of the function of the rods removes what seemed at one time to be formidable objections to the Young-Helmholtz theory of the visual processes[1]. It is therefore incumbent on those who accept Hering's theory or some modification of it, to offer some explanation of the phenomena dealt with above equally satisfactory with von Kries' explanation and less difficult to reconcile with Hering's theory of the visual processes.

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[1] In a paper recently published (*Proc. Roy. Soc.* 1905, B. Vol. 76) Dr Burch describes experiments which seem to prove that for his eye there is no photo-chromatic interval for light of any wave-length. Since the coloured field observed by him was probably in every case so small as to fall well within the limits of the rodless foveal region of the retina, his conclusion must be taken to apply to that region only. It is therefore perfectly compatible with von Kries' view of the functions of the rods, while it is not to be reconciled with Hering's theory of the visual processes.

ON A NEW METHOD FOR THE STUDY OF CONCURRENT MENTAL OPERATIONS AND OF MENTAL FATIGUE.

BY W. McDOUGALL.

(*Preliminary Communication.*)

The conditions to be satisfied. The apparatus and method of using it. The application of it to the study of (1) *concurrent operations,* (2) *of fatigue, etc. Examples of results of experiments on fatigue and effects of drugs.*

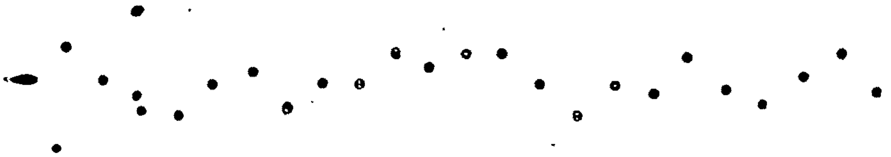
"THE discovery of a reliable measure of the attention would appear to be one of the most important problems that await solution by the experimental psychology of the future." The truth of this dictum of Prof. Külpe has received practical recognition in the numerous experimental researches that have been directed towards the solution of the problem. These experiments, commonly called experiments on the distraction of the attention, have usually taken the form of asking the subject to perform two mental tasks concurrently or intercurrently, (*e.g.* to add continuously figures spoken to him while he recites or reads verses, or to write one verse while he recites another), or of comparing, (in respect of quantity and accuracy), work done under the influence of disturbing sensory stimuli with work done in the absence of such stimuli. Hitherto all such methods have had the great drawback that they neither guarantee the uninterrupted direction of attention to one task nor provide an objective record of breaks in the continuity of the attention-process that would enable us to study the way in which the attention is distributed between the two tasks or oscillates rapidly from one to the other.

It is obvious that if these defects can be removed and a method can be devised that will provide the subject of experiment with a task demanding for its execution a continued maximal voluntary concentration of attention, that will give an objective or graphic record of any failure of continuity of attention and also an objective measure of the accuracy with which the task has been performed, such a method will be applicable to a large number of interesting problems, including the problems of mental fatigue and recuperation and of the influence of drugs upon capacity for mental work. The apparatus described below enables us to set the subject a task which satisfies these conditions, or at least more nearly satisfies them, I believe, than any other that has hitherto been devised.

The method was evolved by a considerable amount of experimentation carried on in the psychological institute of the University of Göttingen, and I have to tender my hearty thanks to Prof. G. E. Müller for valuable advice in connexion with this work.

The main part of the apparatus (which was made by Messrs Diedrichs of Göttingen) consists of a brass cylinder (60 cm. in circumference, 30 cm. in length) mounted horizontally on a steel axis which is put in regular rotatory motion by clockwork. The clockwork is driven by a heavy weight (some 9 kilos.) pulling on a cord wound on a small drum, which transmits, by means of systems of cog-wheels, a rapid motion to a pair of wind-sails and a slower motion to the large cylinder. It is important that the wind-sails are so large and their rotation so rapid that by far the larger part of the work done by the falling weight is absorbed in overcoming the large and very constant resistance of the air. The rate of rotation of the cylinder is varied by adding to or diminishing the weight which drives the clockwork, by means of small plates of lead. The large cylinder is covered with a sheet of white paper of which the overlapping ends are joined by strips of gummed paper. The paper carries eight parallel rows of small red spots, each row surrounding the cylinder in an annular fashion. The subject sits before the cylinder, and, as it rotates, sees the rows of red spots carried vertically downwards. His task is to mark each spot in one of the eight rows with the point of a stylograph containing black ink. It is important to use for this purpose a spring-pointed stylograph such as the Pelican stylo. made by Messrs De La Rue & Co., which never fails to leave its mark when brought in contact with the paper.

The red spots are about 1·5 mm. in diameter, and in each row they succeed one another at a vertical interval of 5 mm. Hence, since the rate of rotation of the cylinder is constant during the passage of any one row, the time interval, at which the spots succeed one another at any given horizontal line, is constant and is a function of the rate of rotation of the cylinder. The spots of any one row form, not a straight line, but an irregular zigzag line. Hence the subject has to *aim* the point of his stylograph at each spot, moving it a little to one side or the other as he strikes at each oncoming spot. Since the cylinder is 60 cm. in circumference and the spots of each row are 5 mm. apart in the annular direction, there are 120 spots to each row, and during one complete rotation of the cylinder the subject has to aim the point of his stylograph 120 times, and has the same fraction of a second for the accomplishment of each aimed stroke. In order that the difficulty of reaching the spots shall not vary from spot to spot, it is necessary to confine the lateral deviation of the spots from the straight line within certain limits. After trial, I adopted the following limits—the extreme lateral deviation of the spots in any one row is 10 mm., but no spot deviates from the line of its predecessor by more than 5 mm.; hence no lateral movement of the point of the stylograph of more than 5 mm. is necessary in making any one movement. Each row of spots was mapped out on paper ruled in 5 mm. squares, and was carefully plotted so that the succession of the spots should be as *irregular* as possible, without the lateral deviation of any one spot exceeding the prescribed limits (see figure). Each of the eight rows was made different from the rest, so that it should be impossible for the subject to learn the order of deviation of the spots. The eight parallel rows, mapped out in accordance with these principles, were then lithographed in red ink on a large number of sheets of smooth paper cut to fit the cylinder. The rows of red spots being designed in this way, the task of the subject is one of uniform difficulty throughout the marking of a row moving at a given rate; and the difficulty of the tasks depends upon the rate of movement of the cylinder.



The third essential part of the apparatus is a black metal screen covering the front of the cylinder and having a horizontal slit of variable width through which the subject sees and marks the spots as they appear in the slit. The screen consists of a lower and an upper piece. The former is screwed firmly to the framework which carries the cylinder. Its upper horizontal edge lies parallel to the surface of the cylinder, separated from it by an interval of 1 mm., and lies in the same horizontal plane as the axis of the cylinder. The upper piece is a light strip of black metal which is hung on adjustable supports at its two ends so that its lower edge, parallel to and a little above the level of the upper edge of the lower piece, lies lightly against the paper covering the cylinder. The presence of this screen prevents the subjects from seeing at any moment more than the short part of a row of spots appearing in the slit or horizontal space between the upper and lower pieces. The width of the slit may be varied according to the nature of the experiment. I have found a width of 10 or 15 mm. to be favourable. The wider the slit (within limits) the greater the rate at which the cylinder may rotate without the subject ceasing to be able to mark each spot. The use of this screen produces greater regularity of results, and is especially important in experiments in which it is desired to secure continuity of direction of attention; for, if a considerable length of the row of spots is exposed to the eye, the eye may run ahead of the hand and take in the positions of a certain number of oncoming spots, and the hand may then continue to mark them while the attention is momentarily directed elsewhere.

In studying the concurrent or intercurrent performance of two different mental operations, the subject is first set to mark the spots while the cylinder rotates at such a rate that he easily puts his black mark on each red spot. The rate of rotation is then increased step by step until a maximal effort of attention alone enables the subject to mark each spot. He may then be set any other task that does not involve the use of his eyes or of his right hand, *e.g.* the addition of figures dictated to him, the counting of irregular taps, the discrimination of two points touched on the skin, the performance with the fingers of the left hand of simple or compound reactions to auditory signals.

It is desirable in such experiments to make upon the moving paper some graphic record of the moment at which any signal or stimulus is given to the subject, and of the moment at which he performs any desired reaction. For this purpose the following simple arrangement was added to the apparatus. A tubular glass pen is carried by a metal holder which slides upon a steel rod fixed parallel to the surface of the cylinder. The brass point of the pen lies upon the clear margin of the paper in the space between the lower and upper pieces of the screen, and, as the cylinder rotates, the pen traces a straight line upon the paper. The pen is held between a silk thread and a stretched india-rubber band. Any push or pull upon the silk thread then causes a lateral deviation of this straight line, which serves to indicate the moment at which any signal or reaction is made; *e.g.* a five-fingered reaction-key may be so arranged for the left hand of the subject that a reaction made by any finger causes a momentary deviation of the pen; while the moment of the signal may be similarly recorded. At the conclusion of the experiment one then has graphic records of the performances of the right and left hands respectively running parallel to one another on the paper, in which any two points on the same transverse line represent the same moment of time during the revolution of the cylinder; the degree of reciprocal interference of the two processes, the marking of the spots by the right hand and the making of reactions by the fingers of the left hand, may then be studied at leisure and correlated with the introspective observations of the subject. In such a case one usually finds clear evidence of interference; *e.g.* if the reaction has been made by the left hand, one or more spots will have been left unmarked by the right hand owing to the momentary deflection of the attention from the task of the right hand to that of the left. It will be seen that by a little ingenuity a great variety of experiments can be made. A number have been made and the results, objective and introspective are interesting, but a prolonged series of experiments with different subjects is necessary for the establishment of general conclusions.

The apparatus may also be used for the study of the onset and passing away of fatigue, excitement, and practice, or of the influence of fatigue, drugs, rest, etc. upon capacity for mental work, or for studying the changes in this capacity that take place from hour to hour, from day to day, and from month to month.

A few short series of experiments of this kind, sufficient I think to prove the value of the method, may be quoted:

After daily practice at marking, carried on until no further improvement of rate was appreciable, the cylinder was set to rotate at such a rate that I was not able to mark all the spots accurately. I then marked a whole paper, *i.e.* the eight rows, continuously at 8.15 a.m. and repeated this at intervals throughout the day, making always a maximal effort to mark all the spots accurately[1]. The errors of marking were then reckoned according to the following principles: The complete omission of a mark, or an extra mark between two spots (a common form of error), was reckoned one error, a lateral deviation of the mark from its red spot of more than 1 mm., or a vertical deviation of more than 2 mm., was reckoned half an error. The total errors of each paper were made out, with the following results:

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[1] The visceral effects of this sustained effort are very striking, but have not yet been accurately studied.

TABLE I.

(23·3″)	paper marked	at	8.15 a.m.,	total errors	80
(23·[illegible]″)	„	„	8.30 a.m.,	„	107
(23·8″)	„	„	8.45 a.m.,	„	111
(23·8″)	„	„	9 a.m.,	„	125
(23·8″)	„	„	9.15 a.m.,	„	116
(9.30 a.m. breakfast with little very weak tea.)					
(23·7″)	paper marked	at	10.15 a.m.,	total errors	66
(23·7″)	„	„	1.15 p.m.,	„	74
(1.30 lunch, no alcohol.)					
(23·1″)	paper marked	at	3.15 p.m.,	total errors	65
(23·8″)	„	„	5.15 p.m.,	„	72
(23·4″)	„	„	7.15 p.m.,	„	99
(7.30 dinner, no alcohol.)					
(23·6″)	paper marked	at	9.15 p.m.,	total errors	109
(23·7″)	„	„	11.15 p.m.,	„	157

In this as in the following series the width of the slit was kept constantly at 15 mm. The rate of rotation of the cylinder (indicated by the figures of the first column of the Table) varied between 1 revolution in 23·8″ and 1 revolution in 23·1″. It was found impossible to keep the rate of rotation absolutely constant, but it was shown by independent experiments that a difference of rate of as much as 1 sec. per revolution made no clearly appreciable difference in the number of errors.

The series repeated on another day gave closely parallel results. It is to be noted that the system of reckoning the errors does not bring out the full differences in accuracy of performance, nevertheless we have very wide differences in the numbers of errors made at different times of the day, the numbers ranging from 65 to 157, and showing that the method affords a delicate measure of the changes of one's capacity for mental work that take place throughout the day.

To illustrate the application of the method to the measurement of the effects of drugs on capacity for work the following series may be quoted:

TABLE II.

On Oct. 7 (normal series).

(24·7″)	paper marked at	6.30 p.m.,	total errors	60
(25″)	„ „	6.45 p.m.,	„	85
(24·7″)	„ „	7 p.m.,	„	107
(24·7″)	„ „	7.15 p.m.,	„	96
(24·7″)	„ „	7.30 p.m.,	„	91

On Oct. 8 (alcohol series).

(24·7″)	paper marked at	6.30 p.m.,	total errors	74

(At 6.37 took 3 oz. of whiskey in warm water.)

(24·9″)	paper marked at	6.45 p.m.,	total errors	144
(25″)	„ „	7 p.m.,	„	146
(24·6″)	„ „	7.15 p.m.,	„	154
(24·8″)	„ „	7.30 p.m.,	„	139

On Oct. 9 (tea series).

(24·5″)	paper marked at	6.30 p.m.,	total errors	70

(At 6.40 took 2 breakfast-cups of good tea.)

(24·6″)	paper marked at	6.45 p.m.,	total errors	77
(24·5″)	„ „	7 p.m.,	„	84
(24·6″)	„ „	7.15 p.m.,	„	61
(24·8″)	„ „	7.30 p.m.,	„	51

A second similar series with weaker doses of the drugs gave parallel results as follows:

TABLE III.

Oct. 14 (alcohol series).

(24·7″)	paper marked at	6.30 p.m.,	total errors	100

(At 6.48 took 1 oz. whiskey in warm water.)

(24·6″)	paper marked at	6.50 p.m.,	total errors	95
(24·6″)	„ „	7.10 p.m.,	„	111
(24·6″)	„ „	7.30 p.m.,	„	145

Oct. 15 (tea series).

(24·6″)	paper marked at	6.30 p.m.,	total errors	98

(At 6.40 took 1 breakfast-cup of good tea.)

(24·6″)	paper marked at	6.50 p.m.,	total errors	123
(24·5″)	„ „	7.10 p.m.,	„	92
(24·7″)	„ „	7.30 p.m.,	„	76

Oct. 16 (normal series).

(25″)	paper marked at	6 p.m.,	total errors	82[1]
(24·8″)	„ „	6.20 p.m.,	„	94
(24·6″)	„ „	6.40 p.m.,	„	96
(24·6″)	„ „	7 p.m.,	„	108

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[1] The lower total of errors here was probably due to the fact that the series was begun half-an-hour earlier than the foregoing series, at a time of day when, as Table I shows, my power of work rapidly falls, or it may have been the expression of one of the inexplicable daily variations which, as other series of experiments showed, may be of considerable amount.

The effects of fatigue and of the drugs, tea and alcohol, seem to be brought out very strikingly by these figures. If we take the totals of errors under alcohol and tea, and without drugs for the two series we have

TABLE IV.

	Normal	Alcohol	Tea
First series (4 papers)	379	583	273
Second series (3 papers)	298	351	291

The effects of the drugs appear very clearly, yet the system of reckoning the errors does not fully bring out the differences of performance; a more rigid and laborious system would show still larger differences. An experiment may be quoted to show the influence of sleep.

TABLE V.

Oct. 12. Paper marked at 6.45 p.m. (rate 24·6″), errors 108.
(Slept from 7 p.m. to 7.30 p.m. and felt partially refreshed.)
Paper marked at 7.45 p.m. (rate 24·6″), errors 85.

It would have been more satisfactory to have performed the work without being aware what drug, or if any drug, had been taken. But this is difficult to arrange, and I do not think that expectation determined in any degree the differences of accuracy of the performances. In every case a maximal effort was made, and in several cases the results were opposed in character to those expected, *e.g.* in the first alcohol series I had expected to find a slight initial improvement after taking alcohol, but a great diminution of accuracy resulted.

Perhaps the most striking result is one in which clear evidence of fatigue was unexpectedly revealed after the conclusion of a series of experiments by inspection of the numbers of errors committed. This series, in which I played the part of subject, was planned by Dr Rivers, who kindly allows me to quote the following figures.

Two papers were marked at 8 a.m. and 8.30 a.m. on each of nine consecutive days. On three days the 8 rows of each paper were marked continuously; on three days an interval of rest, equal to the time of one complete revolution of the cylinder (a little less than half-a-minute), intervened between the completion of each row and the beginning of the succeeding row; on three days an interval equal to the time of two revolutions intervened. These three procedures alternated with one another on successive days. The papers marked at 8 a.m. being designated α, and those at 8.30 a.m. β, the following totals of errors were found:

TABLE VI.

	No interval	1 rev. interval	2 revs. interval
Errors of	3 α papers = 92	3 α papers = 83	3 α papers = 110
,,	3 β papers = 155	3 β papers = 102	3 β papers = 114
Excess of errors in β papers	= 63	19	4

That is to say, the β papers, marked half-an-hour after the α papers in each case, show in all three procedures a larger number of errors than the α papers, indicating in each case the persistence after half-an-hour of the fatigue produced by the marking of the α paper.

This excess of errors is very small in the case of papers of the third group in which an interval of rest of nearly one minute elapsed between the marking of each two rows, is rather larger in the second group in which the intervals of rest were only half as long, and is very much larger in the case of the first group in which the eight rows of each paper were marked continuously. This seems to prove clearly that to mark the eight rows continuously produces a degree of fatigue such that after half-an-hour it remains still considerable, that the fatigue from this procedure is much greater than when the brief intervals of rest intervene, while when the longer intervals intervene the fatigue produced is so much less as to have disappeared almost completely after half-an-hour.

If this series stood alone one might be inclined to regard the result as a chance coincidence. But in the afternoon of each of the same nine days two papers were marked, one (α) at 2.30 p.m., the other (β) at 3 p.m., the same three procedures as in the morning series being adopted, one on each of the three days. And this series gave results very closely parallel to those just quoted, thus:

TABLE VII.

	No interval	1 rev. interval	2 revs. interval
Errors of	3 α papers = 63	3 α papers = 39	3 α papers = 29
,,	3 β papers = 94	3 β papers = 51	3 β papers = 34
Excess of errors in β papers	= 31	12	5

This method, then, enables us to measure, after an interval of rest of half-an-hour's duration, the degree of fatigue produced by an effort sustained for about three minutes only.

It will be noticed that the totals of errors of the afternoon series are only about half as large as the totals of the morning series, and this difference between the errors of the morning and afternoon series appears in the results of the work on each of the nine days. This amply confirms the conclusion, indicated by the series made at intervals throughout one day (Table I), that in the early afternoon my capacity for high-pressure work is much greater than before breakfast, a fact previously unsuspected by me.

Although very few persons have hitherto been subjected to this test, there are indications that the method will serve to bring out individual peculiarities very clearly. Thus one subject, *K*, improved very rapidly with practice, so that after marking only 16 rows at successively faster rates he reached a degree of efficiency (as measured by rate and accuracy of performance) that was reached by two other subjects only after much longer practice. Again, a fatigue curve may be plotted by taking the numbers of errors in each of the eight rows of one paper (continuously marked) as the ordinates of the curve, or better, by taking, as ordinates, the averages of errors in the rows 1, 2, 3, etc. respectively, of a number of papers, marked by the subject under similar conditions. Thus the averages of errors of the eight rows of four papers were in case of two subjects, *A* and *B*, who had had about the same amount of practice, as follows:

TABLE VIII.

row	1	2	3	4	5	6	7	8
A	3	7	7	12	15	18	26	42
B	3	6	10	8	8	11	13	12

This difference in the rate of onset of fatigue may be brought out strikingly by adding the totals of errors for the first four and last four rows respectively, of several of the papers marked by each subject. We then find in the case of the two subjects *A* and *B*:

TABLE IX.

Total errors (averages of 4 papers).

	rows 1—4	rows 5—8
Subject *A*	29	101
Subject *B*	27	44

These figures seem to imply a marked difference between the two subjects as regards the power of sustaining a maximal effort of concentration.

After daily practice at marking continued for some weeks it was evident on introspection that the process of marking the spots had become in some degree mechanical. This observation was confirmed objectively in two ways: (1) in the later experiments the fatigue curve rises rather less rapidly than in the earlier, *i.e.* there is smaller increase of errors in the later rows of each paper marked continuously; (2) it was found that, at an early stage of practice, the threshold of discrimination of two points touched on the left forearm was raised much above the normal (approximately doubled) when determined during the marking of the spots, whereas after long practice at marking this threshold was but little raised by the simultaneous marking of the spots.

This tendency of the process to become mechanical is clearly a drawback to the method. It may, I think, be in great measure obviated by setting the subject the task of marking alternate spots only. This procedure has the further advantage that the process involves a more complex perceptual activity. The subject has to perceive not only the position of each spot marked but also its relations to its neighbours. The process is in fact so much more complex that just about twice as much time is required for its accomplishment as for the simpler task of marking each one of the spots; for after considerable practice of both procedures I found that I was just able to mark the rows in either manner without error at the same rate of rotation of the cylinder. The fatigue produced and measured by this procedure is therefore a more purely central fatigue and is less complicated by fatigue of the muscles and of the lower motor centres. For these reasons I would recommend the adoption of this modification of the procedure in any applications of this method.

The results described above suffice, I think, to show that we have here a delicate method for investigating problems of this kind, perhaps a more delicate and more readily applicable method than any previously devised. I have to thank Dr Rivers for his valuable cooperation in working out the application of this apparatus to the investigation of the effects of fatigue, drugs, etc. I have his permission to mention that he has devised a new form of the apparatus which will render it possible to continue the marking of the spots without interruption for a long period of time.

PROCEEDINGS OF THE PSYCHOLOGICAL SOCIETY.

January 28, 1905. Some Peculiarities of Memory, more especially as regards the Elements of Space and Time, by SOPHIE BRYANT.

The reciprocal development of the Visual Directive and Tactile Motor Executive Faculties in relation to Cerebral Localisation, by F. W. MOTT.

(1) The effect of the mode of feeding in the development of binocular stereoscopic vision, as illustrated by carnivora compared with herbivora; the visual cortex of the cat compared with that of the rabbit; the pyramidal tract of the spinal cord of the cat compared with that of the rabbit. Marsupials—the dasyure and the wallaby.

(2) The effect of dissociation of the fore limbs from progression, and the reciprocal simultaneity in the development of touch and binocular stereoscopic vision.

(*a*) In the apes—lateral and convergent eye movements independent of head and neck movements, perfect binocular stereoscopic vision and translation of visual into tactile motor impressions with the development of a complex associative memory of form, extension, solidity and qualities of bodies. A correlation of structure and function shown by a general expanse of the neo-pallium—phylogenetically new cortex subserving the functions of vision, hearing and tactile motor sensibility as distinct from the archipallium or old cortex subserving the functions of smell—covering up the cerebellum. This expanse is largely due to the formation of a definite occipital lobe, bounded in front by the 'Affenspalte,' the surface of which is smooth. The grey matter forming the whole cortex of this lobe resembles in its cell and fibre structure the grey matter of only a certain portion (calcarine region) of the human cortex. It is much more complex in structure than that of all other animals lower in the scale than primates. Development of annectant gyri for association of hearing and vision—the judgment of direction.

(*b*) In anthropoid apes—more complete dissociation of the fore limbs from progression with development of erect posture; vision becomes a more important directive faculty in gait and station; and a more complex motor adaptation is required to maintain the erect posture. A corresponding change occurs in the convolutional pattern of the cerebral cortex, shown by an increased development and complexity, of the frontal and parietal regions. In the frontal region there is now a definite area of cortex for eye movements, separated from the region connected with head movements by an inexcitable area. The increased development of the parietal lobe has had the effect of pushing back and infolding the smooth cortex of the occipital lobe met with in the ape, and there is an increased development of the visuo-psychic associational zone.

(*c*) In man—the erect posture and specialisation of the right hand together with the function of speech has had a marked influence in the great increase of the cerebral cortex, especially the development of the association centres of the frontal, temporal and parietal regions. The great development of the parietal lobe is as much a characteristic feature of the human brain as is the frontal development. The effect of the development of the parietal lobe in pushing back the primary visual centre round to the mesial surface of the occipital lobe. Significance of the appearance of the two occipito-parietal annectant gyri not met with in the anthropoid apes. Persistence of the anthropoidal ape type of occipital lobe in some low types of humanity and imbeciles. A significant fact that this is more frequently met with in the left hemisphere.

April 1, 1905.	A table of cases to illustrate the typical varieties of Volition and of other allied Mental States, by A. F. SHAND.
	A study of Rhythm among Primitive People[1], by C. S. MYERS.
July 8, 1905.	Some relations of Intelligence to Instinct and to Rational Thought, by C. LLOYD MORGAN.
	Observations on the Brain of the chimpanzee, Sally, by F. GOTOH.
	Exhibition of an Apparatus for registering Fluctuations of Attention, by W. McDOUGALL.
November 4, 1905.	Experiments on Association, by BEATRICE EDGELL.
	The Hipp Chronoscope, by BEATRICE EDGELL and W. LEGGE SYMES.

These observations were undertaken to determine the conditions, and degree of accuracy of the chronoscope, for measurement of reaction time.

For this purpose we have employed time intervals of known duration, furnished by a Wundt's control-hammer in the chronoscope circuit, taking series of chronoscope readings of the hammer time, with different adjustments of the chronoscope, and determining the hammer time itself by the chronographic method, using a Pfeil-Kronecker electro-magnetic signal, and a tuning-fork vibrating 200 (or 100) times per second.

The chronoscope on which the observations were made, was manufactured by Messrs Peyer and Favarger of Neuchâtel, successors to the original maker of the instrument.

It is well known that the vibrating spring (escapement spring) of the later models tends to change its octave, whereby the chronoscope gives occasional readings of half the proper magnitude. We have practically eliminated this, by readjusting the escapement. Otherwise we have left the escapement untouched, preferring to control the hands by the spiral springs attached to the armature. We have taken ten chronoscope readings of the hammer time, at each position of these springs, determining their arithmetic mean and mean variation, and reversing the chronoscope current after each reading.

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[1] Part of this communication is published in the present number.

Occasional omission to reverse the chronoscope current gives rise to no great irregularity. Repeated omission, however, may lead, as is well known, to greater variation in the readings, especially when the hands run in the absence of current. Moreover, reversal after a series of omissions is often followed by an anomalous reading. We have therefore adopted reversal after each reading, as an invariable practice.

It was found important, in comparing chronoscopic and chronographic readings, to adjust the currents (of the chronoscope and chronograph), so that the chronoscopic readings of two intervals of time should have to one another the same ratio as have the chronographic readings of the same intervals. Neglect of this adjustment may give rise to gross error in the readings.

The mean variation of the chronoscope readings does not vary with their length. Variations are therefore due, rather to irregularity in starting and stopping, than to irregularity in the running of the mechanism, and are proportionally greater in short intervals than in long ones. This is also true of the chronograph.

With the lower magnet (actuated by a current of 50 milliampères) releasing the hands, and the left spiral spring arresting them on cessation of the current, the chronoscope recorded the 'true' (*i.e.* the chronographic) value of the hammer time with mean variations less than 1^{σ}.

With the right spiral spring releasing the hands, and the upper magnet (actuated by 50 milliampères) arresting them, true readings were given with mean variations about double the foregoing, and at no constant difference in tension of the spiral springs.

In all cases it was found preferable to derive the chronoscope- (and chronograph-) current, from high electromotive force, through high resistance. For a given current —the greater the resistance, the more constant were the readings; presumably on account of the smaller extra current involved.

Irregular arrest of the small hand has been noticed,

i. When arrest is effected by the left spiral spring—if this spring is insufficiently stretched.

ii. When arrest is effected by the upper magnet—if the opposing spring is in excessive tension.

Owing to elastic fatigue of the springs, the first case may be met with at the end of a long series of readings, though absent from its commencement.

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